THE

UNIVERSITY OF IOWA

STUDIES IN PSYCHOLOGY

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Researches upon School Children and College Students.

BY

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The researches presented in this paper may be considered as a continuation of the list of problems worked out by me on the New Haven school children and published in the *Studies from the Yale Psychological Laboratory*, Vol. II, under the title "Researches on the Mental and Physical Development of School Children."

The following data were taken in the Public Schools of Iowa City, West Liberty, and Cedar Rapids, Iowa City Academy, and the State University of Iowa. To the superintendents, principals and teachers of the schools mentioned I am under obligation for their kind sympathy and assistance. To Dr. Patrick, Professor of Philosophy in the University, I am indebted for many helpful suggestions and services.

The taking of the experiments has extended over the greater part of two academic years. A large portion of the data on the ages from 6 to 15 were taken by three of my pupils, Miss May Henry, Mr. H. F. Kallenberg and Mr. C. C. Stover, during the academic year of '95–'96, the uncompleted ages to nineteen inclusive being taken by myself during the year '96–'97. The number of children in the Iowa City schools being insufficient to complete the required number, the apparatus was moved to the schools mentioned above.

By comparing the list of tests given below, with those taken in New Haven, it will be seen that four or five have been repeated both to verify the results themselves and also

to test further the mooted question as to the relation of physical development to mental ability. It would be well if there could be more verification of results already obtained. It is true that that means a great deal of work without marked advance except in accuracy, but above all things the modern movement in child study is in need of care and accuracy. The most discouraging feature of all investigations in psychology and preëminently in child study is the large variation we have to face not only among the results given by the various experiments, but also among the individual data offered by the person experimented upon. So, in the list of tests given below will be found a number of tests already taken by me in New Haven.

When the problem was begun it was the intention to include in the experiments all ages from 6 to 21 inclusive, but it was found impossible to get a sufficient number of subjects of the ages 20 and 21 from the resources at our disposal, so that the tests were carried only through age 19.

List of Tests Taken.

Each person was tested is the following respects: (1) Pulse both before and after the series of tests. (2) Pain threshold. (3) Strength of lift with the wrist. (4) Strength of lift with the arms. (5) Estimation of length by arm movement. (6) Estimation of length with the eye. (7) Lung capacity. (8) Weight. (9) Height. (10) Voluntary motor ability. (11) Fatigue.

Methods and Apparatus.

Test (1): *Pulse.*

Before beginning the series of tests the pulse of each child was counted for one-half minute. Subsequent to the series of tests the pulse was taken again so as to get the effect on the heart of the labor represented by the tests and in particular by the last one, viz., voluntary motor ability, inasmuch as the child was required to tap consecutively for forty-five seconds in order to produce fatigue, as will be explained below. The

pulsations were counted as usual at the wrist. The period of one-half minute was chosen in preference to the minute interval so as to get the maximum rate of heart-beat after fatigue, for the frequency of the heart-beat decreases very rapidly immediately after violent exercise.

TEST (2): *Pain Threshold.*

The apparatus used in this test which might, for convenience, be called the balance algometer was constructed from a balance scale. Upon the one scale pan was placed a block with a small round strip so arranged as to press directly against the finger nail when placed between the block and a stationary bracket above. Thus when the opposite scale pan was pressed down, the one on which the block was placed moved up, pressing the nail of the finger between the block and the stationary bracket. The index-finger of the right hand was used. The greater the weight placed in the opposite scale pan the greater the pressure upon the nail of the subject. The fact that the finger was placed up-side-down between the block and the bracket precluded any possibility of the pressure being removed from the nail by downward pressure of the finger. Thus the finger, subjected to pressure, was simply allowed to lie in a relaxed state, increased pressure finally causing a stinging sensation under the nail. In order to be able to control exactly the amount of pressure exerted upon the finger, small lead cylinders were made, each weighing fifty grams. Pressure on the nail was increased gradually by dropping into the opposite scale pan one cylinder of fifty grams each second, asking the subject to indicate the time when pressure began to pass over into pain, or in other words, when it began to hurt. The metronome beating seconds was used to mark the time for dropping in the lead cylinders, one being dropped at each click. There is a definite point of transition from pressure to pain, the former giving way to the latter in the form of a sharp pricking sensation, such as when the finger is cold or pricked with a pin. The pressure being brought to bear on the nail instead of on the flesh of the finger

obviates the difficulty of errors creeping in, due to difference in callosity of the finger, this error being less with the nail than elsewhere.

TEST (3): *Wrist Lift.*

The strength of lift with the wrist was taken with Mauriaud's spring dynamometer with handle for lifting and hook for supporting the load. By a self-recording index finger the amount lifted is indicated on a scale in kilograms. In order to use the dynamometer to the best advantage, a chain with small links was fastened firmly to the end of a board upon which the subject for experiment took his stand. By means of the links of the chain the height of the dynamometer from the floor could be varied to suit the height of the child by hooking the load-supporting hook of the dynamometer in the different links of the chain.

In standing on the board preparatory to the lift, the subject was simply asked to place his feet in position similar to prints already placed on the board. These prints were placed at right angles to each other, the left foot to the rear and the right foot placed at right angles to the left with the heel of the right foot against the instep of the left. Placing the elbow of the subject against the hip with the forearm at right angles to the upper arm the handle of the dynamometer was placed against the under surface of the elbow and the load-supporting hook of the dynamometer hooked in the link which just stretched the chain tight. By this means all subjects were subjected to exactly the same height of lift relative to their height in stature. With the elbow still planted against the hip the first three fingers of the subject's right hand were placed in the handle of the dynamometer, and he was then asked to lift all he could keeping the body erect, thus giving a free arm lift which was largely dependent on the strength of the wrist where the strongest strain was felt. It might be stated that the first three fingers were used instead of four because the handle of the dynamometer was not sufficiently large to admit four fingers of some of the larger adult hands.

TEST (4): *Lift with Arms.*

The dynamometer used in this test was constructed in the laboratory using a "Chatillon" spring scale suspended between two upright supports, the force of the lift being brought to bear upon the scale by means of pulleys through which a rope was passed from the scale to the two handles grasped one in each hand. The pulleys necessarily introduced considerable friction. To insure the accuracy of the results the dynamometer was tested and corrected by inverting the dynamometer and suspending a box from the handles. By adding weight to the box and recording the same, the scale on the face of the spring scale was corrected to the actual amount of weight hanging on the handles which would, of course, correspond to the amount lifted by the subject during the experiment. Thus, not only the friction of the ropes in the pulleys, but also the inaccuracy of the spring scale itself, in case there were any, was corrected. The strength of lift was recorded in pounds as indicated on the scale and subsequently changed into kilograms as they are represented in the charts and tables below. When lifting the subject took his stand between the two ropes and, with straightened back and knees bent, lifted as hard as he could after the height of the handle had been adjusted to his own stature. The height of the handles could be changed by lowering or raising the scale to which the rope was fastened, thus giving the subject the most advantageous position for a hard lift.

TEST (5): *Estimation of Length by Arm Movement.*

The apparatus consisted of a board thirty-two inches long and eleven inches wide, supported on a table twenty-six inches from the floor. On one end of the board was nailed a sm[illegible] upright board. At a distance of twenty-four inches from th[illegible] was marked plainly a dot, the horizontal board bein[illegible] [illegible] off in inches and eighths of an inch to either side of [illegible] The subject and experimenter being [illegible] on oppos[illegible] of the board, the subject was ask[illegible] [illegible]

carefully the distance from the vertical board to the dot, his eyes were covered with a card and he was then asked to move the pencil along on the board and put the end of it on the dot as nearly as possible. His error in judgment was then marked in inches and eighths of an inch plus or minus according as he went too far or fell short of the line drawn through the dot at right angles to the direction of motion of the pencil. Each subject was given five trials without telling him his error till after the set was completed. The pencil used was of wire inasmuch as marking was unnecessary, the error being taken simply from where the pencil was brought to a rest. In order to insure uniformity in the way the pencil was held by all, a washer three-eighths of an inch in diameter was soldered to the wire pencil, one half inch from the end. The subject was asked to hold it like a pencil with his index-finger against the washer thus allowing no error to enter due to the different length of the pencil from the hand to the board.

In calculating the result, the five trials of each subject were averaged, and the mean of these averages found for the final average which was then reduced to centimeters before tabulating and inserting in the charts.

TEST (6): *Estimation of Length by Sight.*

On a board twenty-nine inches long and three and one-half inches wide, were marked two heavy lines, each one inch long and twenty inches apart. The subject while standing five feet from the board was asked to estimate in inches as nearly as he could the distance between the two lines, they being the only marks on the board. In order to have the board at the height of the subject's eyes, and thus uniform for all, it was suspended from the moveable arm of the measuring rod used in taking the height and, the height being taken previously in the series, the board for this test was always at the exact height of the subject's eyes. The estimation by the subject was recorded in inches and fractions of an inch, and reduced to centimeters preparatory to insertion in the charts.

TEST (7): *Lung Capacity.*

The lung capacity was taken on Barnes' standard wet spirometer which consists of a cylindrical vessel nearly filled with water, through the center of which a tin tube projects, connected below with a rubber tube through which the subject emptied his lungs forcing the air into the inner chamber which is formed by a water-tight and air-tight tin cylinder. By means of a rubber stopper the air blown into this inner chamber may be removed and the cylinder lowered to its zero mark ready for a second test. An index finger pointing to a scale on a support on one side marked off in cubic inches is fastened to the moveable cylinder. As the air is blown into the inner chamber the cylinder rises, the number of cubic inches being indicated by the height to which it rises. The weight of the tin cylinder is balanced by weights hanging on pulleys above, so that very little pressure is required to raise the cylinder by blowing.

The child was told to inhale all the air his lungs would contain and then exhale it through the tube into the spirometer, emptying his lungs as completely as possible. After having found the mean value for the different ages the final results were reduced to cubic centimeters.

TEST (8): *Weight.*

The weight was taken on a Fairbanks' platform scale with a capacity of 400 lbs., and weighing to an accuracy of 2 oz. Ordinary indoor clothing and shoes were worn by the subjects. The scales were tested each time after moving.

TEST (9): *Height.*

The rod by which the height was taken was fixed stationary, attached firmly to the upright of the scales upon which the weight was taken. Upon the upright rod was fastened a Keuffel & Esser millimeter scale and tested before the measurements began. To avoid the possibility of the wood shrinking or other accident, it was tested also several times during the progress of the measurements to insure its

correctness, but such tests proved unnecessary in that it remained accurate. A moveable arm was fastened upon the rod at right angles to it. In taking the height the subject was placed under the moveable arm. After sliding it down till it struck the head, the height was recorded in centimeters. The height was taken with shoes.

TEST (10): *Voluntary Motor Ability and Fatigue.*

The apparatus used in recording the rapidity of tapping was constructed in the laboratory from the alarm movement of a common alarm clock. Having substituted a key finger-rest for the hammer of the alarm and reversing the spring, the scape movement was so set that by tapping the key the hand set on a face on the outer surface of the box in which the movement was encased, moved one point. By means of a spring beneath the key the time of tapping could be regulated at will.

The subject was asked to tap as rapidly as he could until told to stop, and was allowed to tap for forty five seconds. The metronome beating seconds was used to measure the time. Immediately after the subject had started tapping, the spring holding the key was released and kept open for a period of five seconds and then closed, thus stopping the recording of the taps. At the end of the forty-five seconds another period of five seconds was allowed, the number of taps made the first five seconds having been recorded during the interim of forty seconds.

It is, of course, impossible to tap as rapidly after tapping forty-five seconds as when fresh and unwearied. The number of taps the first five seconds would express the voluntary motor ability. The per cent. of loss in rapidity of tapping would express the fatigue due to that amount of tapping. If the number of taps for the first five seconds be represented by r and the number made the last five seconds be represented by s, then g, the degree of fatigue for forty seconds of tapping, will be expressed by

$$g = \frac{r - s}{r}$$

In tapping the arm was held free from the table, giving a free arm movement.

General Methods.

There were always three subjects in the experimenting room, so that while one was taking the tests the other two could be watching, and thus when their turn came but little time was required for explanations. By this means also they became acquainted with their surroundings, which is no small item in the younger ages. The following order was adopted in taking the tests. 1. Pulse; 2. Pain threshold; 3. Wrist lift; 4. Weight; 5. Height; 6. Estimation of length by sight; 7. Estimation of length by arm movement; 8. Lung capacity; 9. Lift with arms; 10. Voluntary motor ability and fatigue; 11. Pulse after fatigue.

When one child had completed the series of tests, he went to his room, another coming to take his place. The cards upon which the data were taken were of different colors for the different sexes to facilitate sorting according to sex. Before the experiments were taken the cards were filled out in the following particulars, either by the teacher, or parent, or by the subject himself, if of sufficiently advanced age. 1. Name; 2. Age; 3. School; 4. Grade; 5. Birthplace; 6. Birthplace of father; 7. Birthplace of mother; 8. Father's occupation; 9. Date. Numbers 5, 6, and 7 were taken for the nationality of the subject, and number 8, to classify the child as to home, surroundings and the like.

Results.

About one hundred of each age were tested. The slight variation from this number will be found expressed in the tables below under the discussion of the separate tests. In calculating the results the mean value was used in preference to the arithmetical average.[1]

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[1] Scripture, *On the Adjustment of Simple Psychological Measurements, Psych. Rev.*, 1894, I. 281.

In order to express the homogeneity of my results the mean variation was calculated for all ages and all tests for boys and girls separately.

After the record-cards were filled out and the experiments taken, the cards were returned to the teachers of those tested and marked with a figure 1, 2, or 3, according to what she judged the child's mental ability to be, marking the bright ones "1," those of average ability "2," and the dull ones "3."

This method is without doubt more accurate than simply to take the standing of the child from the record of his examination grades, for, at best, examination grades are inaccurate and frequently do not express the mental precocity of the child, whereas the teacher under whom the children have been working, can without difficulty separate the pupils into the three grades mentioned. At least for the purpose of the problem in hand this method has the advantage.

The aim in having the cards marked thus was to get by a separate calculation the relation between the general mental ability and the respective tests.

TEST (1): *Pulse.*

Inasmuch as this test depends somewhat on the tests for voluntary motor ability and fatigue it will be treated subsequently to those tests.

TEST (2): *Pain Threshold.*

In tests taken on pain, the time element has generally been neglected, but by experiment it has been found that the rate at which pressure is added influences greatly the amount required to produce pain. However, though this difficulty was regulated as far as possible by adding a definite amount of pressure, viz., 50 grams each second, the mean variation is large.

The results of the test are shown in Table I and represented in graphic form by Charts I and II. The figures to the left of the charts indicate the number of kilograms necessary to be added at the rate of 50 grams per second to produce pain, the

subject being asked to announce the fact as soon as it commenced to hurt.

TABLE I.

Pain Threshold.

Age	*PB*	*MV*	*PG*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	1.26	.50	1.15	.35	1.07	1.15	1.73	91	43	48
7	1.38	.48	.93	.50	1.30	1.30	1.20	96	46	50
8	1.70	.70	1.18	.40	1.28	1.30	1.81	93	49	44
9	1.69	.60	1.36	.42	1.34	1.52	1.65	100	52	48
10	1.67	.64	1.45	.48	1.43	1.44	1.93	108	47	61
11	2.07	.53	1.56	.48	1.97	1.80	1.85	97	52	45
12	2.00	.52	1.46	.49	1.85	1.64	1.73	111	54	57
13	2.05	.66	1.70	.50	1.80	1.86	1.90	101	51	50
14	2.13	.52	1.82	.43	1.99	1.98	2.03	92	48	44
15	2.35	.82	1.77	.46	2.16	2.11	1.80	95	50	45
16	2.70	.68	1.85	.33	2.12	2.40	1.87	92	44	48
17	2.75	.69	1.93	.52	2.10	2.18	2.17	100	50	50
18	2.85	.71	1.80	.51	2.05	2.45	2.20	100	50	50
19	2.78	.57	1.75	.60	1.90	2.56	2.27	81	50	31

PB, kilograms of pressure for boys.
MV, mean variation in kilograms for boys.
PG, kilograms of pressure for girls.
MV', mean variation in kilograms for girls.
A, kilograms of pressure for bright subjects.
B, kilograms of pressure for average subjects.
C, kilograms of pressure for dull subjects.
N, number tested
NB, number of boys.
NG, number of girls.

On Charts I and II, the figures to the left indicate the number of kilograms. The ages are marked along the line of abscissas. The results show a gradual decrease of sensibility to pressure as a rule from 6 to 19, boys being less sensitive than girls throughout. Girls have reached nearly the minimum of sensibility by the time they are 13 years of age, while for the boys that age marks the point at which the most rapid falling off in sensitiveness to pressure seems to begin, so that up to the age 14, the difference remains nearly the same for the two sexes at the different ages, the average difference being about .4 kilograms, but subsequent to that time the difference increases until at 19 there is a difference of more than a kilogram between the sexes. This phenomenon is of

interest when taken in connection with the results of some of the other tests to be dealt with below.

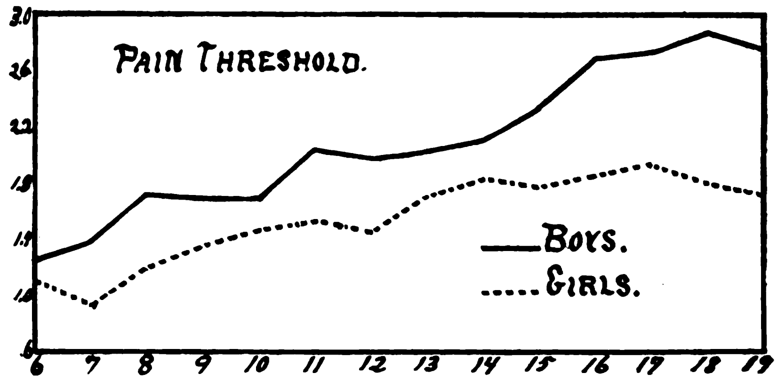

CHART I.

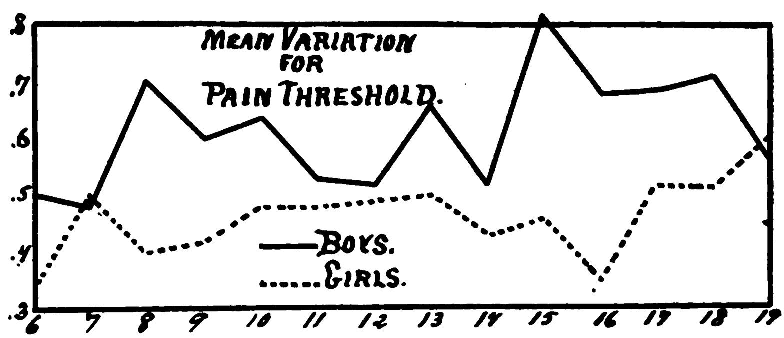

CHART II.

All pain tests seem to present the difficulty of large mean variation and the present test offers no exception to the rule At age 15 for boys it reaches the height of 820 grams or .82 kilograms, the lowest mean variation being .33 kilograms at the age 16 for girls. While the effect of age is to produce a gradual and for the most part a regular decrease in the sensibility the mean variations observe no such regularity in change but vary from age to age, apparently regardless of any laws except that the mean variation is less for girls than for boys but that is as would be expected inasmuch as the pain threshold is lower for girls than for boys.

Test (3): *Wrist Lift.*

The data on this test are given in Table II and presented in graphic form in Charts III and IV. The increase in strength is very marked and regular throughout the development of the child, showing however the same marked dividing point in the rapidity of development at the age 14 for both sexes, boys beginning their most rapid increase at that point, while girls begin slightly to retard their rate of development. As can be seen by reference to Chart III, boys have a greater strength than girls at all ages, the difference not being so marked till age 14, but at age 19 a boy lifts just about twice as much as a girl. At 6 the difference is only 0.5 kilograms, at 14, 3.5, and at 19, 13.0 kilograms.

Table II.

Wrist Lift.

Age	*LB*	*MV*	*LG*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	4.5	1.0	4.0	.9	4.0	4.5	4.5	91	43	48
7	6.2	1.3	5.0	1.1	5.9	5.0	6.0	96	46	50
8	7.0	1.5	6.0	1.3	6.4	6.5	6.5	93	49	44
9	8.1	1.5	6.0	1.1	7.4	7.5	7.0	100	52	48
10	9.8	1.6	7.5	1.5	7.2	8.8	7.8	108	47	61
11	10.3	1.6	8.0	1.8	9.8	10.0	10.5	97	52	45
12	12.0	1.5	8.1	1.8	9.9	10.5	11.8	111	54	57
13	13.0	2.5	10.0	2.2	11.6	11.0	12.0	101	51	50
14	14.1	2.4	10.6	2.5	12.8	12.0	13.0	92	48	44
15	18.7	4.2	11.8	3.2	20.0	14.3	14.0	95	50	45
16	22.0	4.0	12.4	2.6	16.5	16.0	13.2	92	44	48
17	25.3	3.7	12.8	2.0	17.5	17.0	21.5	100	50	50
18	25.3	4.5	14.0	1.5	20.0	15.4	19.8	100	50	50
19	27.0	3.1	14.0	1.4	15.5	26.3	22.6	81	50	31

LB, kilograms lifted by boys.
MV, mean variation for boys.
LG, kilograms lifted by girls.
MV', mean variation for girls.
A, kilograms lifted by bright subjects.
B, kilograms lifted by average subjects.
C, kilograms lifted by dull subjects.
N, number tested.
NB, number of boys.
NG, number of girls.

The figures at the left of the charts represent the number of kilograms lifted with the wrist, the figures at the bottom giving the age.

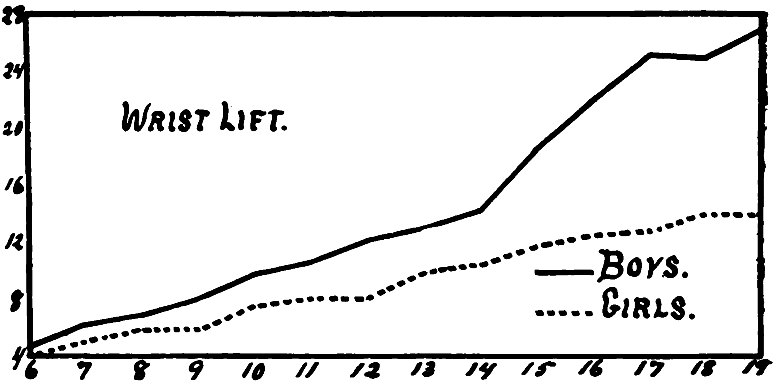

CHART III.

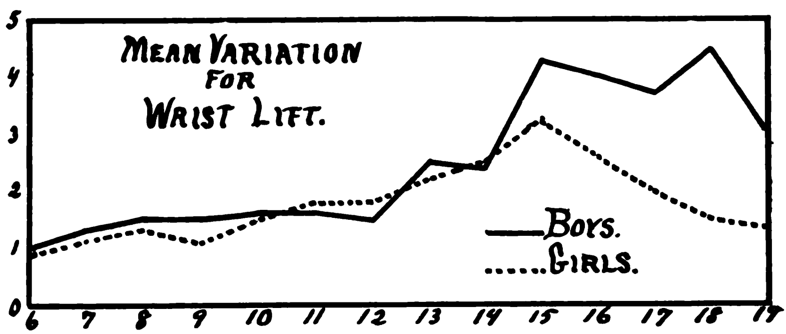

CHART IV.

By reference to Chart IV it will be seen that the mean variation begins to decrease at the same age at which there is the change in rapidity of growth for both sexes, as is shown in Chart III. Here as a rule also the mean variation is less for girls than for boys, largely because of the less strength shown by the former.

TEST (4): *Lift with Arms.*

The results of this test follow almost precisely the same law as was manifested in the previous test for wrist lift. The data tabulated in Table III are also presented in Charts V and VI. The girls have reached almost their maximum strength at the age 14, whereas, the boys begin to increase in strength the

most rapidly at that age. Here also, it will be noted, the average lift for boys is just about twice the amount lifted by girls at the age 19. At age 6 there is a difference of only 8.4 kilograms. At 14 it is 30.9, while at 19 there is a difference between the sexes of 83.5 kilograms. Boys, as would be judged, are stronger than girls throughout the entire span from 6 to 19. The figures to the left of the charts represent the number of kilograms for the respective ages given at the bottom.

TABLE III.

Arm Lift.

Age	*BL*	*MV*	*GL*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	33.8	4.5	25.4	4.4	28.1	32.2	28.1	91	43	48
7	42.2	8.6	32.2	4.4	38.1	32.7	39.5	96	46	50
8	51.7	9.9	36.3	8.6	42.6	45.8	50.8	93	49	44
9	58.9	10.4	45.8	7.3	53.1	54.4	47.6	100	52	48
10	68.5	9.9	44.5	9.9	56.7	58.5	53.5	108	47	61
11	78.0	9.9	52.2	9.9	60.8	72.1	69.9	97	52	45
12	85.7	10.4	58.5	10.4	73.5	73.5	71.2	111	54	57
13	94.3	13.1	69.8	12.2	73.5	81.2	88.5	101	51	50
14	107.1	13.1	76.2	16.8	84.8	90.7	96.2	92	48	44
15	130.1	17.7	81.6	16.3	126.5	127.5	96.2	95	50	45
16	138.4	23.6	78.4	10.9	116.1	88.5	92.1	92	44	48
17	158.3	20.9	81.2	13.2	103.4	113.9	128.3	100	50	50
18	171.9	24.5	82.6	15.9	118.4	107.1	117.5	100	50	50
19	171.9	16.8	88.4	10.4	120.2	152.9	156.4	81	50	31

BL, kilograms lifted by boys.
MV, mean variation for boys.
GL, kilograms lifted by girls.
MV', mean variation for girls.
A, kilograms lifted by bright subjects.
B, kilograms lifted by average subjects.
C, kilograms lifted by dull subjects.
N, number tested.
NB, number of boys.
NG, number of girls.

The mean variations for strength of lift with the arms show also about the same general law as the mean variations for the preceding test, increasing throughout the period of most rapid growth and then decreasing from then till 19.

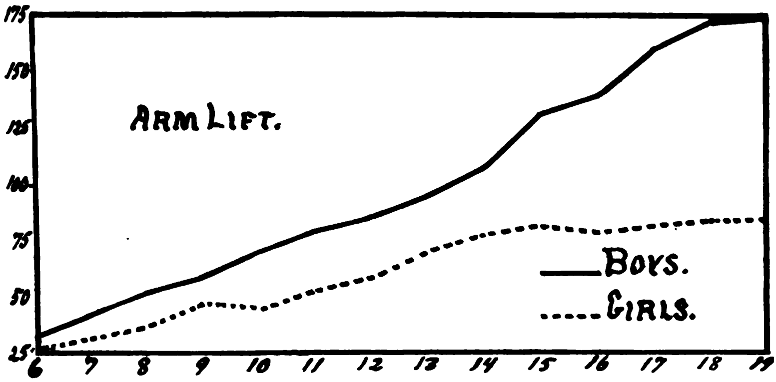

CHART V.

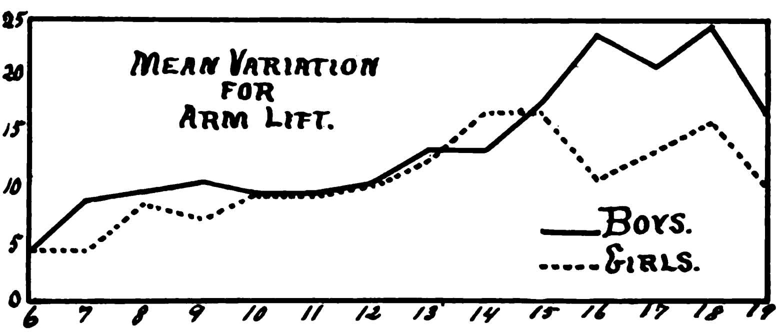

CHART VI.

TEST (5): *Estimation of Length by Arm Movement.*

The accuracy with which space is judged in terms of movement with the arm, said space having been previously estimated with the eye, increases with age. The results of the test are tabulated in Table IV and expressed graphically in Charts VII and VIII. As stated under the description of apparatus for test (5) the board was marked off in inches and the data changed into centimeters after finding the mean values. The figures to the left of the chart represent the number of centimeters the subjects stopped short of the dot, the dot being at a distance of 62 centimeters from the point of starting. At no age did they over-estimate the distance, going beyond

the dot. Comparatively few even of the older ages went farther than the proper distance, so that under such circumstances it might be said that we under-estimate distance translated from the sense of sight to the muscle sense. Boys are less accurate than girls from age 6 to 10. Then the reverse is the case with one exception till age 19, boys becoming more accurate than girls. There is a gradual increase in accuracy with age, with one slight exception in the case of girls at age 11.

TABLE IV.

Estimation of Length by Arm Movement.

Age	*MB*	*MV*	*MG*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	10.9	4.6	8.9	4.3	8.7	10.4	11.4	91	43	48
7	7.9	4.6	6.9	3.0	6.6	8.6	8.4	96	46	50
8	7.4	3.8	6.1	4.6	6.6	7.9	4.3	93	49	44
9	5.8	3.5	5.1	3.0	5.3	4.8	6.4	100	52	48
10	4.8	3.0	5.1	4.1	5.1	4.8	5.1	108	47	61
11	4.8	3.5	5.8	3.3	3.8	5.1	6.9	97	52	45
12	4.0	2.8	4.8	3.3	5.3	4.1	3.6	111	54	57
13	4.3	3.3	4.3	3.0	4.1	4.3	4.3	101	51	50
14	3.8	2.8	3.3	2.5	4.3	3.1	3.1	92	48	44
15	3.6	2.8	3.6	2.0	2.5	3.8	3.8	95	50	45
16	1.5	2.8	3.1	2.5	3.1	2.0	2.0	92	44	48
17	1.5	2.0	1.8	2.5	1.8	1.8	1.8	100	50	50
18	1.3	2.0	2.0	1.8	2.0	1.5	1.5	100	50	50
19	1.3	1.8	1.3	2.5	1.0	1.3	1.3	81	50	31

MB, error in centimeters by boys.
MV, mean variation for boys.
MG, error in centimeters by girls.
MV', mean variation for girls.
A, error in centimeters by bright subjects.
B, error in centimeters by average subjects.
C, error in centimeters by dull subjects.
N, number tested.
NB, number of boys.
NG, number of girls.

The time element is probably one cause for the under-estimation of the distance. When the distance is judged by the eye it is by one sweep of the eye taken rapidly as a rule. When the same space is traversed with the arm there is that gradual change in position of the arm which would act in the muscle sense somewhat in the same way as filled space for the eyes; as we know, filled space is always over-estimated, so here

by the succession of sensations derived from the arm in motion, the distance through which the arm has passed is over-estimated and we stop too soon. The same principle manifests itself in the estimation of time. "A watched pot never boils."

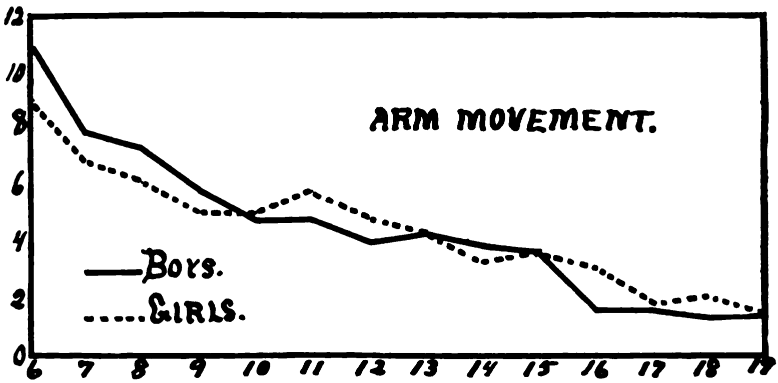

CHART VII.

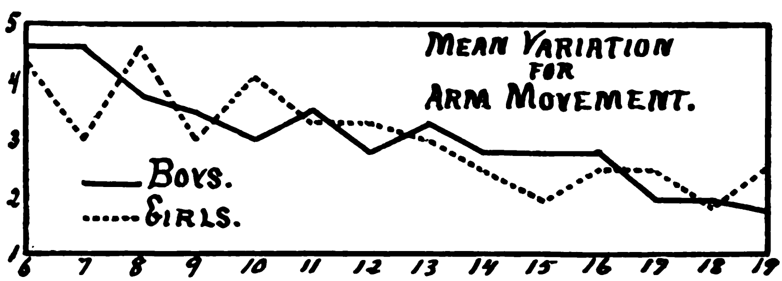

CHART VIII.

The mean variation decreases with age with no indications for comparison between the sexes. At first glance there would seem to be some connection here between height and the accuracy of movement. This will be discussed however below under "general comparisons."

TEST (6): *Estimation of Length by Sight.*

One of the main aims of this test was to find out at what age children begin to make comparatively accurate estimates of spatial dimensions in terms of units. This ability increases very

rapidly from 6 to 11 years of age and more rapidly with boys than with girls. Boys throughout are more accurate than girls with the exception of the ages 6 and 14. At age 6 the child judges the line to be only about one-fifth as long as it really is, estimating it to be 10.7 centimeters when it is in reality 50.8 centimeters long. The inability of the child at the younger ages is evidently due to a lack of knowledge of the inch, and being asked to estimate the line in inches, he is wholly at a loss, not knowing, to start with, what an inch is, so that I received answers everywhere from one inch to fourteen feet. The child does not seem to have a proper conception of what he is to do until nine or ten years of age, girls, it would seem, learning to pass approximately accurate judgment about two years later than boys. Up to the age 15, the distance is always judged shorter than it really is. Subsequent to that time it is judged longer than it really is, the most accurate age being between 15 and 16. However, the line is judged longer and

TABLE V.

Estimation of Length by Sight.

Age	*EB*	*MV*	*EG*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	10.7	27.1	12.7	17.3	14.0	7.6	15.2	91	43	48
7	31.2	21.1	20.3	12.4	30.0	25.9	16.0	96	46	50
8	38.1	19.3	29.7	15.0	36.8	31.5	31.0	93	49	44
9	44.4	15.2	31.8	17.0	38.1	45.2	27.9	100	52	48
10	46.5	10.4	38.9	16.0	43.2	45.2	38.1	108	47	61
11	46.7	12.7	46.5	15.5	50.8	45.7	48.2	97	52	45
12	44.7	9.7	41.1	13.2	45.7	45.2	35.6	111	54	57
13	46.7	6.6	43.2	11.4	45.7	47.5	44.5	101	51	50
14	46.5	6.6	48.3	9.1	46.7	48.3	45.7	92	48	44
15	50.3	5.6	46.7	11.9	51.6	50.3	46.5	95	50	45
16	51.5	4.6	51.1	8.6	51.8	49.5	54.6	92	44	48
17	53.1	4.8	51.0	8.4	50.8	52.8	54.8	100	50	50
18	54.9	6.1	52.1	7.4	51.6	54.9	53.3	100	50	50
19	57.1	5.3	51.5	7.1	52.1	57.1	57.6	81	50	31

EB, estimation of 50.8 centimeters by boys.
MV, mean variation for boys.
EG, estimation of 50.8 centimeters by girls.
MV', mean variation for girls.

A, estimation by bright subjects.
B, estimation by average subjects.
C, estimation by dull subjects.
N, number tested.
NB, number of boys.
NG, number of girls.

longer as age advances. It seems probable, at least, that this is due to the method used in estimating the distance. The older the subject is the more anxious he will be to get an accurate estimate and, as it were, divides the space into separate inches, jumping with the eye inch by inch, instead of estimat-

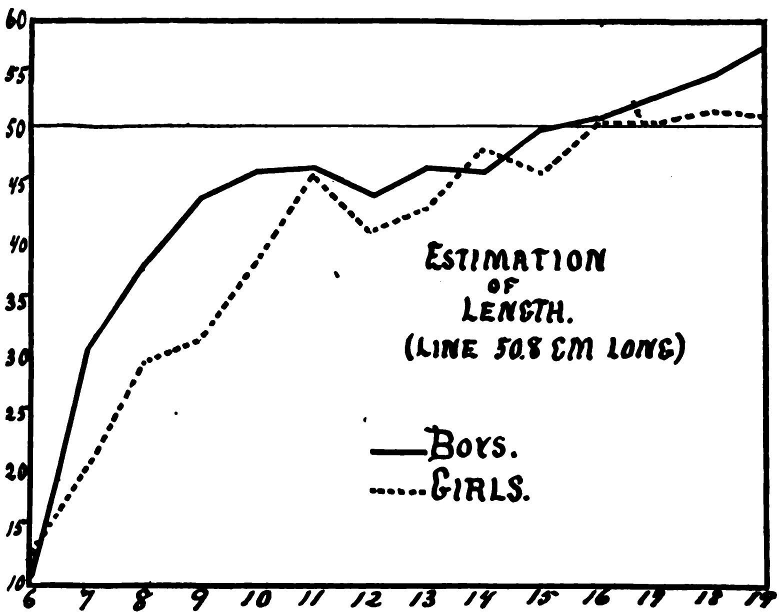

CHART IX.

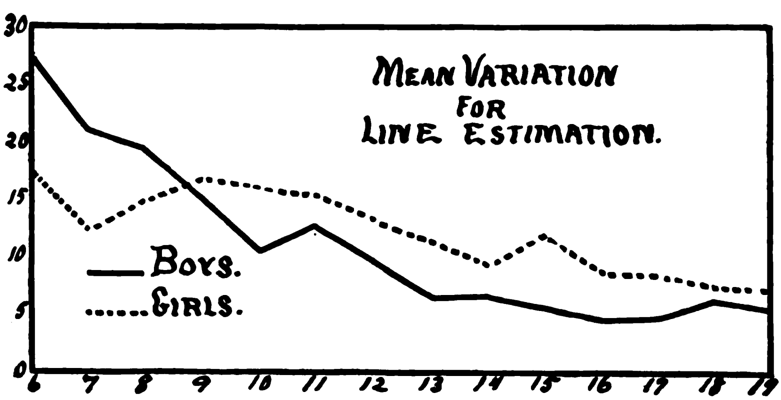

CHART X.

ing it as a whole. In fact some would try to form an estimate by beginning at the left and counting off inch by inch. By the well known principle that filled space is over-estimated, it is probable that this division of the twenty inches into separate units to be counted, increases the estimate of the whole. Should we agree that the older the subject becomes the easier it would be to sweep the eye over the space, then the rule would be reversed and the older the subject becomes the shorter the line should seem on account of less labor expended in traversing the distance. The mean variation decreases for both boys and girls, that of the boys being greater than that of the girls previous to age 9, and less subsequent to that age.

In Chart IX, the figures to the left indicate the number of centimeters long the line of 50.8 centimeters was judged to be by the subjects at the different ages indicated at the bottom. In Chart X the numbers to the left indicate the mean variation in centimeters. The use of the mean instead of the arithmetical average is specially valuable in that it cuts out the influence of all abnormally large and small estimates.

TEST (7): *Lung Capacity.*

The remaining tests were also taken in the series of experiments worked out by me in New Haven and were repeated here in order to discover if possible whether there is any connection between physical development and mental ability. This was specially desirable in weight and height, in regard to which there is some doubt. The data for lung capacity are recorded in Table VI and Charts XI and XII. The figures at the left of the charts indicate the number of cubic centimeters for the respective ages. Boys have a larger lung capacity than girls at all ages. The difference, however, is not so large from 6 to 13 years of age, but subsequent to that time the difference between the sexes increases very rapidly. At 6 the boys have an advantage of 65.7 cubic centimeters. At 13, 283.6, while at 19 years of age the boys have the larger lung capacity by 1610.5 cubic centimeters. Here as

in the lifting tests the girls have reached their maximum at about 13, while at that age the most rapid growth for boys begins.

TABLE VI.

Lung Capacity.

Age	*BC*	*MV*	*GC*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	989.9	98.3	924.2	152.4	885.1	986.7	1007.9	91	43	48
7	1181.7	183.5	1024.4	167.2	1147.3	1065.3	1150.6	96	46	50
8	1396.4	165.5	1155.5	175.4	1311.2	1229.1	1311.2	93	49	44
9	1466.9	206.5	1352.2	134.4	1406.2	1393.1	1442.3	100	52	48
10	1622.6	185.2	1381.7	181.9	1480.0	1475.1	1442.3	108	47	61
11	1991.4	255.7	1491.5	188.5	1729.1	1671.8	1784.7	97	51	45
12	1971.4	244.2	1688.1	237.7	1827.4	1835.7	2065.1	111	54	57
13	2225.8	349.1	1942.2	306.5	2015.9	2024.2	2229.0	101	51	50
14	2622.4	370 4	1950.4	270.4	2097.9	2311 0	2409.3	92	48	44
15	2974.8	420.4	2104.5	272.1	2750.2	2524.1	2266.7	95	50	45
16	3163.3	535.9	2155.3	209.8	2589.6	2327.4	2245.4	92	44	48
17	3810.6	342.5	2097.9	296.7	2655.1	2802.6	3032.1	100	50	50
18	3786.1	508.1	2270.0	367 1	3040.3	2851.8	3310.7	100	50	50
19	3900 1	468.7	2289.6	281.9	2491.2	3507.4	3704.1	81	50	.31

BC, lung capacity in cubic centimeters for boys.
MV, mean variation for boys.
GC, lung capacity in cubic centimeters for girls.
MV', mean variation for girls.
A, lung capacity for bright subjects.
B, lung capacity for average subjects.
C, lung capacity for dull subjects.
N, number tested.
NB, number boys.
NG, number girls.

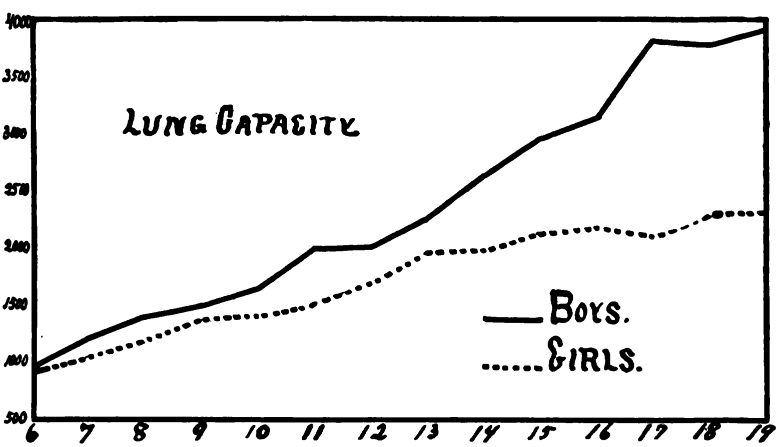

CHART XI.

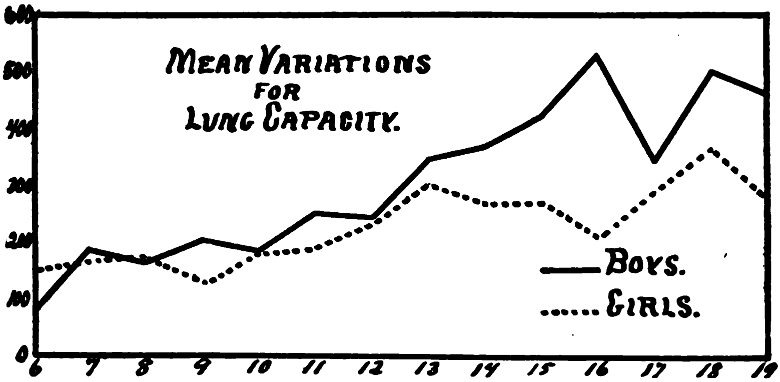

CHART XII.

TEST (8): *Weight.*

The chief object in taking weight and height was not so much for the data themselves, which have been determined by the more extended researches of others, as to offer some further contribution to the problem of the relation of physical development to precocity.

TABLE VII.

Weight.

Age	*WB*	*MV*	*WG*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	20.80	1.7	18.87	1.8	19.05	20.41	20.46	91	43	48
7	23.32	2.8	21.51	2.0	21.64	22.05	24.32	96	46	50
8	24.95	2.2	23.13	2.9	23.69	24.49	24.32	93	49	44
9	27.95	2.5	26.35	2.8	27.45	27.68	26.49	100	52	48
10	28.90	4.1	28.17	2.9	27.96	29.08	28.90	108	47	61
11	32.84	3.7	31.39	4.1	31.57	32.21	32.94	97	52	45
12	35.47	3.6	36.15	4.6	35.47	35.11	40.10	111	54	57
13	41.21	5.5	42.68	7.1	39.01	40.46	45.26	101	51	50
14	46.26	5.0	45.36	6.7	44.54	46.54	44.90	92	48	44
15	53.07	6.9	50.48	5.9	51.26	52.39	50.48	95	50	45
16	58.97	7.4	50.63	5.4	54.25	56.92	52.35	92	44	48
17	63.56	4.8	54.88	4.8	58.24	59.33	61.23	100	50	50
18	64.67	5.7	56.92	3.9	60.90	58.50	59.70	100	50	50
19	65.99	4.8	57.33	5.7	60.10	63.50	66.40	81	50	31

WB, boys' weight in kilograms.
MV, mean variation for boys.
WG. girls' weight in kilograms.
MV', mean variation for girls.
A, weight for bright subjects.
B, weight for average subjects.
C, weight for dull subjects.
N, number weighed.
NB, number of boys.
NG, number of girls.

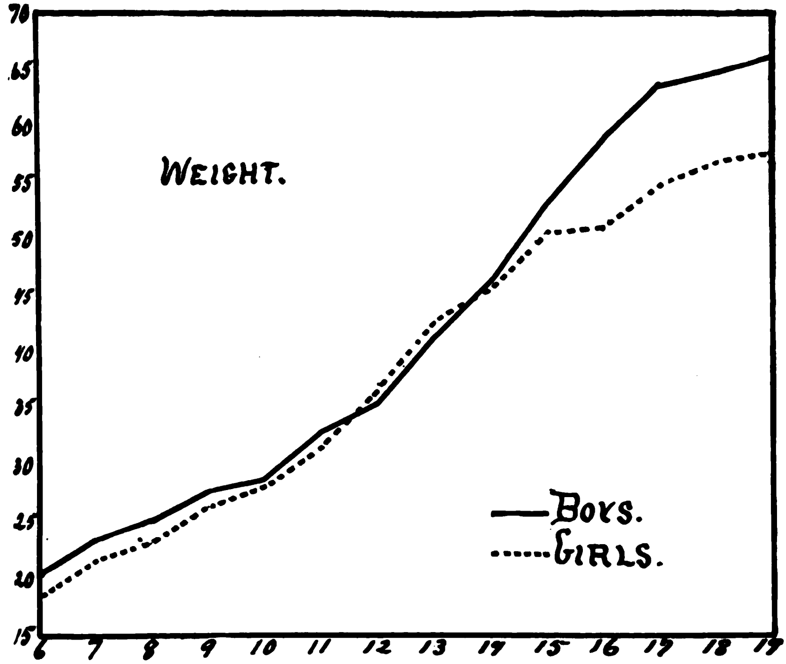

CHART XIII.

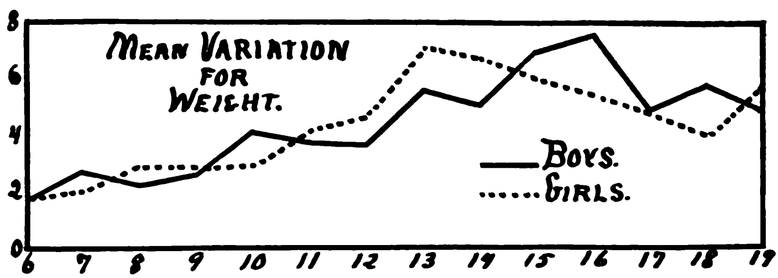

CHART XIV.

The data for weight will be found in Table VII and Charts XIII and XIV. The weights were taken to an accuracy of two ounces. The figures at the left of the charts indicate kilograms.

The same general law is shown here as was found in all previous measurements of children, viz., before the age of 11,

boys are heavier than girls; from the time between 11 and 12 to the time between 13 and 14 the order is reversed and girls are heavier than boys; after this period the order is again reversed and boys are heavier than girls. Girls grow more rapidly from 10 to 15, boys from 12 to 17.

The mean variations are largest in the period of fastest growth, increasing in magnitude till the period of puberty and decreasing thereafter. Previous to age 11 the mean variation is about on an equality for the two sexes. During the period at which girls are heavier than boys the mean variation is larger for the former than for the latter, the reverse being the case when the boys again become heavier than the girls.

Test (9): *Height.*

The height was recorded in millimeters. The results are given in Table VIII and Charts XV and XVI. The figures at the left of the chart indicate the height in centimeters with the ages marked below as usual. The same general laws

Table VIII.

Height.

Age	*HB*	*MV*	*HG*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	114.4	3.9	113.2	3.6	112.4	114.4	116.7	91	43	48
7	121.5	5.3	118.8	3.6	119.4	118.8	121.4	90	46	50
8	126.1	4.1	124.9	4.1	125.5	126.0	124.8	93	49	44
9	130.3	4.6	130.3	3.9	131.0	130.8	128.0	100	52	48
10	135.3	5.7	134.6	5.2	134.4	134.3	134.3	108	47	61
11	140.1	5.1	138.3	6.3	136.8	140.0	141.3	97	52	45
12	145.1	5.4	147.2	5.7	144.4	145.0	151.7	111	54	57
13	149.0	6.2	150.3	7.3	148.8	151.2	154.0	101	51	50
14	156.7	5.1	156.8	5.3	156.4	156.5	158.9	92	48	44
15	164.4	6.7	160.8	4.3	164.3	162.7	163.2	95	50	45
16	169.5	6.9	160.8	3.6	165.0	161.9	163.1	92	44	48
17	173.1	3.6	163.8	4.4	166.7	167.9	169.0	100	50	50
18	174.2	3.3	164.2	3.9	168.0	166.1	169.8	100	50	50
19	175.2	4.4	164.0	3.8	166.4	170.3	172.1	81	50	31

HB, boys' height in centimeters.
MV, mean variation for boys.
HG, girls' height in centimeters.
MV', mean variation for girls.
A, height for bright subjects.
B, height for average subjects.
C, height for dull subjects.
N, number measured.
NB, number of boys.
NG, number of girls.

apply here in regard to rapidity of growth for the two sexes as appeared in the figures for weight. Boys are taller than girls till between 10 and 11 years of age, the girls then become taller till about 14 when the boys again take the lead.

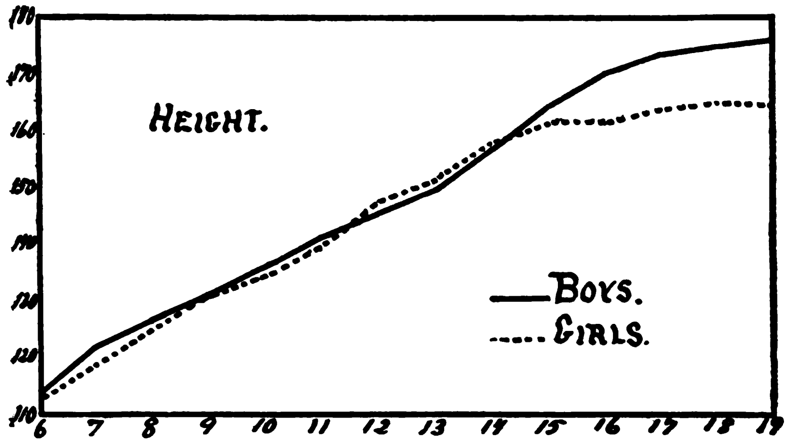

CHART XV.

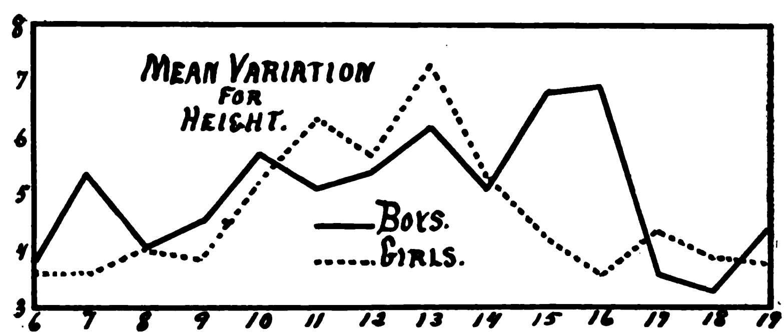

CHART XVI.

The development of girls advances much more slowly after 15 than before that age.

The mean variations show the same law in a general way as was shown in weight, increasing in magnitude till after the period of most rapid growth is past and then decreasing as rapidly as they increased.

Test (10): *Voluntary Motor Ability.*

In Table IX and Charts XVII and XVIII are given the data for voluntary motor ability. The data give the number of taps made in five seconds. For the first three years girls excel the boys, but from then till 19 the boys tap faster than girls. There is a gradual increase in rapidity of tapping for both boys and girls from age 6 to 19, showing an increase of about 15 taps in five seconds, the boys of 6 years tapping 22 times, while the boys of 19 tap 36.7 times in five seconds.

Table IX.

Voluntary Motor Ability.

Age	*MB*	*MV*	*MG*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	22.1	2.1	22.3	2.2	22.1	22.3	21.5	91	43	48
7	23.3	2.7	24.5	2.7	25.2	25.3	21.2	96	46	50
8	25.8	2.4	26.0	2.5	26.0	26.5	24.0	93	49	44
9	27.1	2.4	26.7	2.5	27.0	26.9	26.0	100	52	48
10	28.3	2.6	26.2	3.6	27.1	28.5	27.5	108	47	61
11	28.1	2.2	28.0	3.1	29.0	27.4	27.4	97	52	45
12	30.1	2.9	29.3	2.2	29.7	30.0	27.8	111	54	57
13	31.1	3.8	29.5	2.7	31.0	30.3	29.0	101	51	50
14	32.4	2.9	29.4	2.6	32.0	31.0	28.8	92	48	44
15	34.0	2.6	31.3	2.7	33.4	33.2	31.0	95	50	45
16	34.0	3.1	32.2	3.1	34.0	32.8	33.2	92	44	48
17	34.4	2.2	33.8	3.0	34.0	34.0	34.6	100	50	50
18	36.0	3.1	34.3	2.4	36.1	35.1	35.0	100	50	50
19	36.7	3.3	35.3	3.1	36.2	36.3	36.0	81	50	31

MB, boys—number of taps in five seconds.
MV, mean variation for boys.
MG, girls—number of taps in five seconds.
MV', mean variation for girls.
A, motor ability for bright subjects.
B, motor ability for average subjects.
C, motor ability for dull subjects.
N, number tested.
NB, number of boys.
NG, number of girls.

In the mean variations for voluntary motor ability there is a marked increase just previous to the change of growth for the two sexes, the mean variations for girls being at their maximum at age 10, and at 13 for boys. As is known, the change of growth comes earlier for girls than for boys. The mean variations for fatigue also point to irregularity at this period.

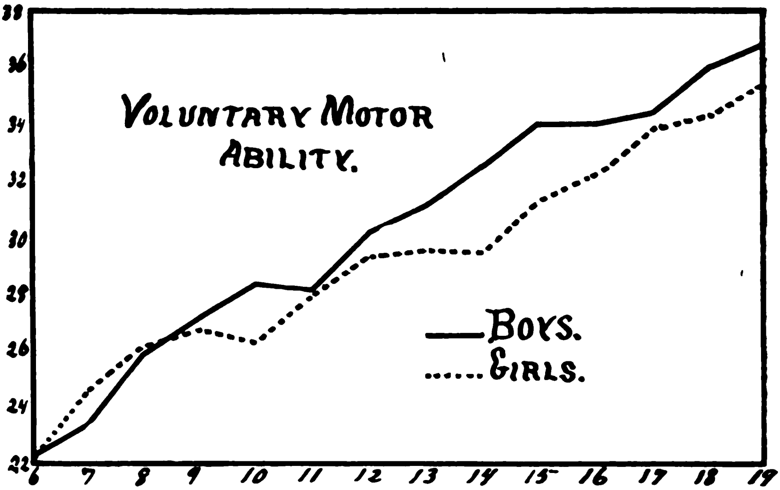

CHART XVII.

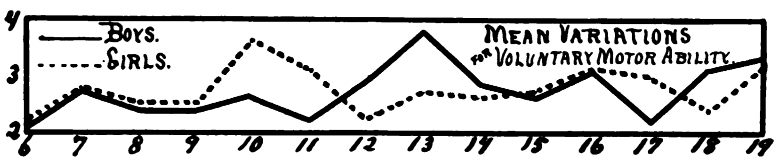

CHART XVIII.

TABLE X.

Fatigue.

Age	*B*	*MV*	*G*	*MV'*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	28.6	5.9	22.4	8.1	27.3	25.6	21.0	91	43	48
7	23.6	6.1	22.2	8.0	25.8	28.9	18.9	96	46	50
8	20.8	8 3	22.9	6.0	24.1	25.3	20.2	93	49	44
9	22.6	7.1	23.3	6.4	23.0	23.8	21.2	100	52	48
10	23.3	6.5	16.6	8.0	17.0	24.2	20.7	108	47	61
11	18.5	7 0	18.5	7.8	21.4	19.0	14.3	97	52	45
12	21.0	6.7	14.8	8.1	18.5	19.3	13.7	111	54	57
13	17.6	5.2	13.8	4.0	18.9	15.0	12.5	101	51	50
14	20.1	7.6	13.2	7.7	19.4	16.1	13.8	92	48	44
15	16.4	6.6	16.6	6.3	17.6	14.8	16.6	95	50	45
16	15.8	5.5	16.2	5.8	17.1	15.8	14.2	92	44	48
17	17.6	5.0	16.6	5.2	18.9	14.9	16.6	100	50	50
18	16.6	5.1	16.9	3.9	17.2	13.9	17.1	100	50	50
19	14.8	5.6	16.6	4.1	13.6	16.1	18.2	81	50	31

B, per cent of loss by fatigue—boys
MV, mean variation for boys.
G, per cent of loss by fatigue—girls
V', mean variation for girls.
A, fatigue for bright subjects.
B, fatigue for average subjects.
C, fatigue for dull subjects.
N, number tested.
NB, number of boys.
NG, number of girls.

TEST (11): *Fatigue.*

After tapping for forty-five seconds the amount of fatigue was calculated and expressed in per cent. of loss in rapidity of tapping. This per cent. of loss for the respective ages is represented in Table X and Charts XIX and XX. The per cent. of loss in rate of tapping decreases with age, but in this series of taps the decrease is so irregular as to preclude draw-

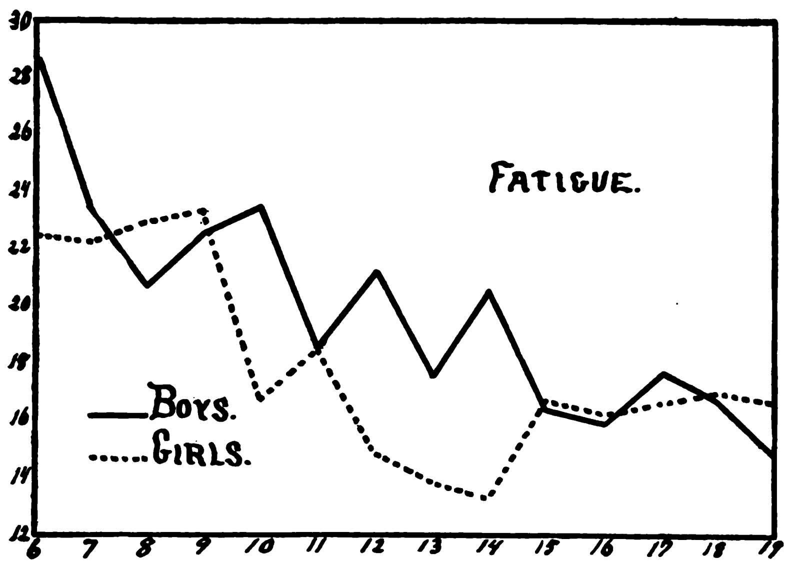

CHART XIX.

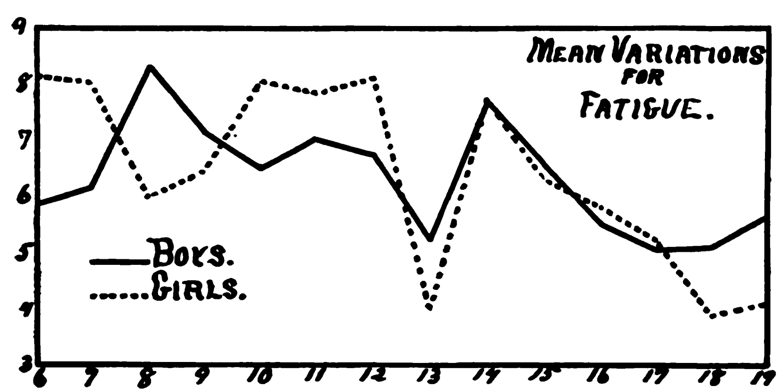

CHART XX.

ing any inference or law other than the general statement that it decreases with age, girls not losing as much as boys. But it will be remembered that boys tapped faster than girls and consequently it would not be expected that they would fatigue so quickly, judging simply from the rate of tapping. In a general way it may also be said that the mean variation decreases with age, but here too the curve is too irregular to point to anything definitely of value for a law of growth or development.

Test (1): *Pulse.*

The data taken upon pulse can scarcely be considered as giving absolute values for the normal pulse of the child. The child seems to be unable to entirely neglect the distracting influence of the novelty of the experiments and hence the pulse is probably higher than the normal when it is first taken. The first counting of the pulse was before making any of the tests. Presumably the work of taking the tests would raise the rate of pulse-beat. However, it was not infrequently the case that the child's pulse was higher at the first counting than at the second, showing that the excitement due to the novelty of the experience exceeded the effect of fatigue in taking the tests. In almost all, however, there was an increase in rate of pulse-beat at the second counting. The plan of having two or three in the room at the same time was adopted largely on this account, in order that the child might become accustomed to his surroundings by the time his turn came. The pulse was counted for only one-half minute, so that the figures in Table XI and to the left of Chart XXI indicate the number of pulse-beats in one-half minute; nor would it be justifiable to multiply the results by two in order to get the pulse per minute, for after fatigue the most rapid beating is during the first half of the minute, the rate decreasing very rapidly after that time. As would be expected, the effect of exercise is to raise the pulse at all ages. Here we find the same general law as that in the curves for weight and height, except that the relations between the sexes are reversed.

TABLE XI.

Pulse.

Age	BP	MV	PB'	MV'	DB	PG	MV"	PG'	MV'"	DG	A	B	C	A'	B'	C'	N	NB	NG
6	53.0	4.7	53.0	4.1	0.0	50.5	4.8	53.7	3.9	2.2	51.7	51.5	53.0	53.0	53.5	53.5	91	43	48
7	49.5	5.5	50.3	5.6	0.8	50 8	4.4	51.8	3.6	1.0	51.2	49.7	50.0	53.7	50.8	51.0	96	46	50
8	47.4	4.4	48.7	4.9	1.3	51.0	5.8	52.5	4.7	1.5	49.5	49.8	48.0	50.1	50.5	49.7	93	49	44
9	45.0	5.3	47.2	4.8	2.2	48.2	5.0	50.5	4.5	2.3	50.0	44.8	46.5	50.3	46.6	47.0	100	52	48
10	44.0	5.0	45.0	4.9	1.0	45.8	5.5	49.6	4.8	3.8	47.0	44.8	43.5	49.2	46.8	47.5	108	47	61
11	44.2	4.1	46.2	4.5	2.0	43.8	5.3	47.0	5.0	3.2	41.5	44.4	45.5	44.0	46.4	48.8	97	52	45
12	44.4	5.7	46.8	5.8	2.4	41.3	5.0	44.2	4.5	2.9	43.5	43.2	42.5	46.0	44.9	44.5	111	54	57
13	45.0	4.3	47.4	3.7	2.4	43.2	4.5	46.0	4.6	2.8	44.0	44.9	44.5	46.2	47.2	46.5	101	51	50
14	43.4	4.3	45.5	4.7	2.1	44.0	4.8	44.2	3.2	1.2	44.7	43.6	43.5	47.0	45.3	45.0	90	48	42
15	41.5	5.0	43.2	6.3	1.7	42.0	5.2	44.8	4.0	2.8	41.3	42.2	40.5	44.0	45.0	43.5	91	50	41
16	42.6	5.3	47.0	5.0	4.4	43.0	4.7	45.6	4.1	2.6	43.3	42.2	43.0	45.0	45.1	46.4	73	33	40

* All figures previous to column *A* represent the number of pulse-beats in thirty seconds.

PB, boys at the beginning of tests.
MV, mean variation for boys at the beginning of tests.
PV', boys at close of tests.
MV', mean variation for boys at the close of tests.
DB, amount of increase by fatigue for boys.
PG, girls at the beginning of tests.
MV", mean variation for girls at the beginning of tests.
PG', girls at the close of the tests.
MV'", mean variation for girls at the close of tests.

DG, amount of increase by fatigue for girls.
A, bright subjects at beginning of tests.
B, average subjects at beginning of tests.
C, dull subjects at beginning of tests.
A', bright subjects at the close of tests.
B' average subjects at the close of tests.
C', dull subjects at the close of tests.
N, number tested.
NB, number of boys.
NG, number of girls.

With the exception of age 6 the boys' pulse is slower than the girls' till between 10 and 11, faster from then till between 13 and 14 and then slower again from 14 on. The taking of the pulse was only carried to age 16. The data point very distinctly to an acceleration of the pulse during the age of puberty for both sexes, both in the curves for normal pulse and pulse subsequent to fatigue. The effect at puberty seems more marked for boys than for girls.

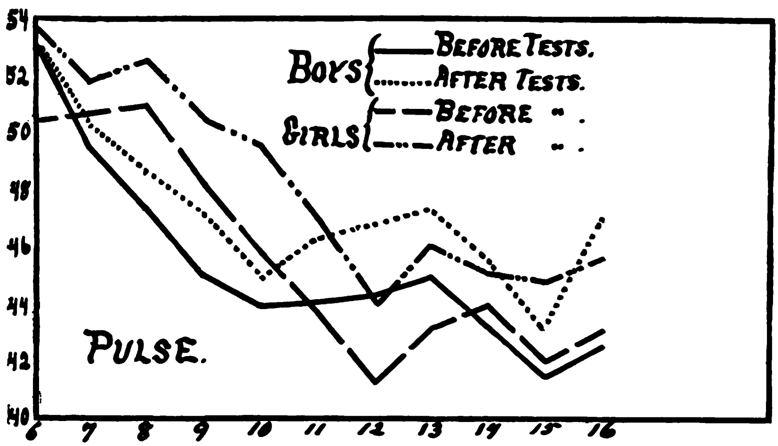

CHART XXI.

Relation of the Different Tests to Mental Ability.

In the solution of this problem is to be found one of the chief aims in taking up the majority of the tests included in the foregoing series. After the tests were completed, the record cards were returned to the respective teachers who were asked to mark each name with a figure 1, 2 or 3, according to what she considered the mental ability of the child to be, marking the bright ones "1," the average ones "2," and the dull ones "3." This is the same method as that which I followed in my New Haven tests in order to avoid the inaccuracy arising from using examination grades as a standard of the mental ability of the subject. Those subjects taken from the University were marked from the University records of their

standing, because no teacher in college work has the student in more branches than one, as a rule. In the Iowa City High School they were marked by the teachers when together at one of their "teachers' meetings."

The results were first calculated for boys and girls, irrespective of the marks relating to mental ability, to get the relation of the two sexes. Subsequently they were calculated with reference to mental ability, averaging those marked 1, 2 and 3, for the bright subjects, those of average ability, and the dull subjects respectively. These data will be found in columns headed A, B and C in the tables for the separate tests given under the discussion of "Results."

The data for most of the tests, when reduced to table and chart form, give so little indication of any direct relation between mental ability and the separate tests that the charts for them have been omitted from the text. In a good many of the tests all indications of any relation existing between physical development and mental precocity seem to be wanting. The following tests, however, give sufficient indication in this respect to call for separate mention: Estimation of length, weight, height, lung capacity, voluntary motor ability, and fatigue.

GRADED ESTIMATION OF LENGTH BY SIGHT.

The data showing the relation to mental ability in this test are to be found in Table V, columns A, B and C. These are given in graphic form in Chart XXII. The figures to the left indicate the number of centimeters judged equivalent to 50.8 centimeters. The light horizontal line would therefore indicate the points of absolutely accurate judgment. It will be seen that except at ages 9 and 13, the bright subjects make a more accurate estimate than the dull ones. Between 1[illegible] and 16 years of age the dull subjects seem to change suddenly from an under-estimation of the length and thereafter over-estimate the true length. After age 14 the b[illegible] averag[illegible] nearly absolute accuracy, the [illegible] averag[illegible] being [illegible] accurate [illegible] years of [illegible]

C

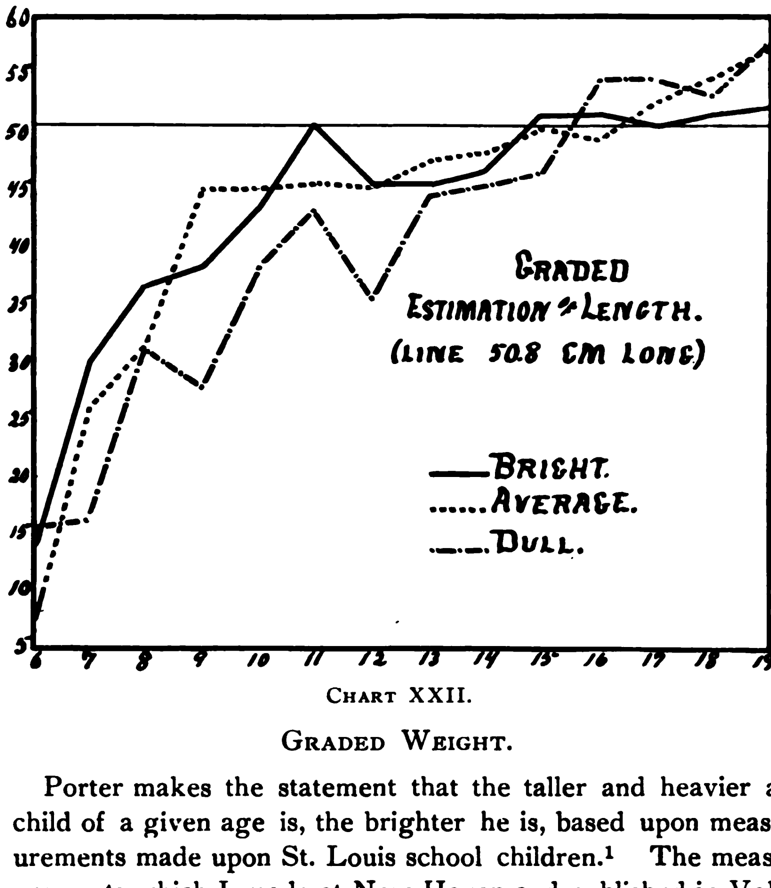

CHART XXII.

GRADED WEIGHT.

Porter makes the statement that the taller and heavier a child of a given age is, the brighter he is, based upon measurements made upon St. Louis school children.[1] The measurements which I made at New Haven and published in Vol. II, of *Studies from Yale Psychological Laboratory* failed wholly to verify Porter's conclusion. The curves cross and recross in such a way as to show no constant relation between physical development and mental ability. The same is largely true of the results obtained in Iowa City and vicinity. If anything could be stated it would be that the heavier and taller children are the duller, but here also the curves cross too frequently to justify any definite statement in regard to any relation so existing. The data are represented in Chart XXIII, the figures

[1] Porter, The Growth of St. Louis School Children. Academy of Science of St. Louis, 1894, VI, 335.

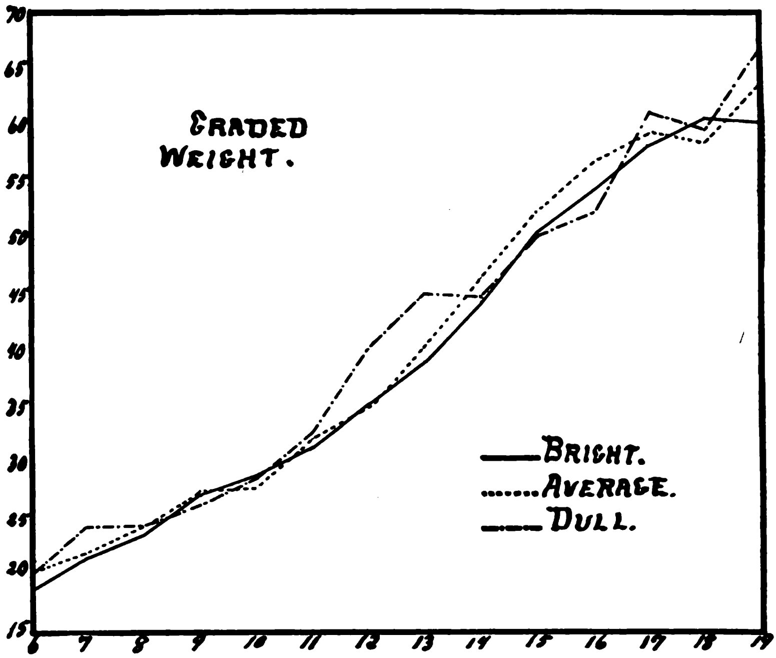

CHART XXIII.

to the left indicating weight in kilograms. During the period from 10 to 14, there is a marked difference, the dull children being much the heavier, while at other ages there is no definite indication. The data may be found in Table VII.

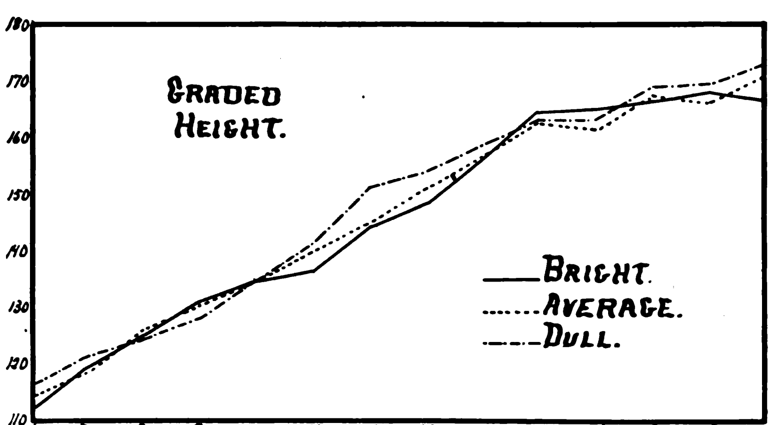

CHART XXIV.

Graded Height.

We find here almost exactly the same results as are expressed in the chart for weight given above. The figures to the left of Chart XXIV indicate centimeters. The data may be found in Table VIII.

Graded Lung Capacity.

The data from which Chart XXV is constructed may be found in Table VI, columns A, B, and C. The figures to the left of the chart indicate cubic centimeters. Here again men-

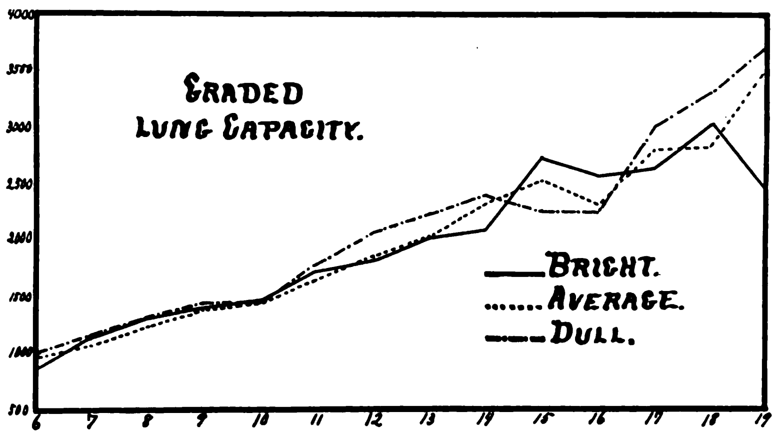

Chart XXV.

tal ability seems to have no definite relation, until age 10. Then during the age of most rapid growth from 10 to 15 the duller children have the largest lung capacity, the distinction vanishing again after that period.

Graded Voluntary Motor Ability.

With the exception of ages 10 and 17, the bright children tap faster than the dull ones, the difference being very marked, as can be seen by reference to Chart XXVI and the data of Table IX, columns A, B, and C.

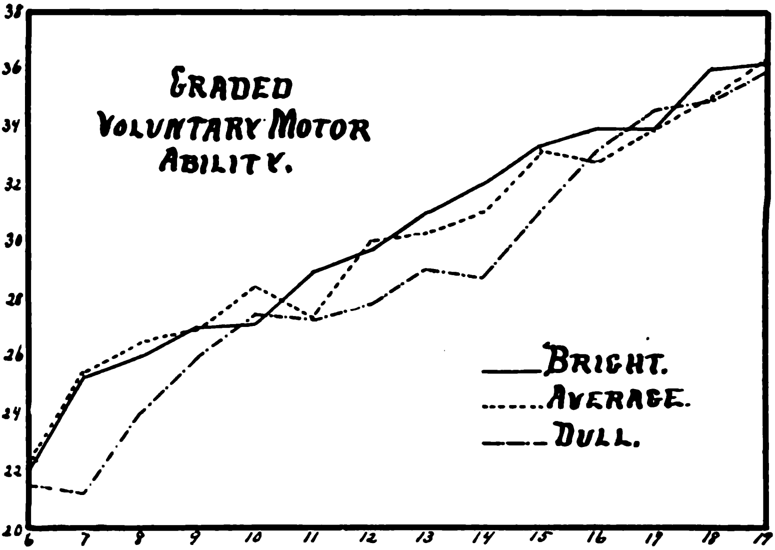

CHART XXVI.

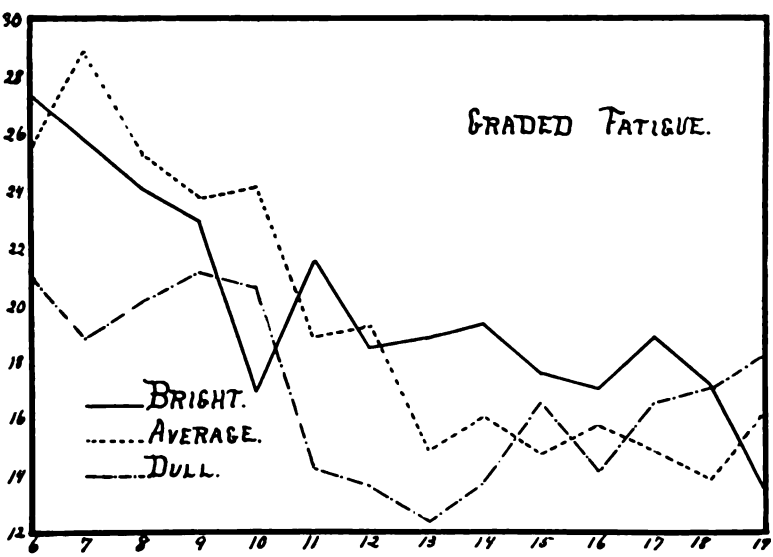

CHART XXVII.

GRADED FATIGUE.

It is to be supposed that the greater the rapidity of tapping

the greater will be the fatigue induced by it. Chart XXVI shows that the bright subjects tap faster than the dull ones, and Chart XXVII marks clearly the fact also that the bright subjects lose more in their rate of tapping by the fatigue induced. The data for fatigue are to be found in Table X, columns A, B, and C. The figures to the left of the chart indicate the per cent. of loss in rate after tapping for forty-five seconds.

General Relations and Comparisons.

In taking a general view of the curves for the separate tests, a large proportion of them show marked changes in the development of the child about the age at which change of growth occurs, viz: from 12 to 16, the most marked effect being at about 14. A glance at the following charts seems to be convincing, in that they are so thoroughly corroborative of this rule. Compare Charts I, III, V, XIII, XV, XXI; also II, IV, VI, XII, XIV. The latter series, it will be seen, are for mean variations and the former for original data on the several tests. Mean variations remain comparatively regular for the two sexes until about age 14, and the change in the variation is largely due to the change in growth coming at that age. Girls seem to complete largely their development a year or two previous to the time at which boys have just begun their most rapid period of development. In pain threshold, arm lift, wrist lift, and lung capacity the girls have about reached their maximum at 14, while at this age the boys begin to develop most rapidly, so that, judging from the present series of tests compared with those taken at New Haven, the statement might almost be risked that a girl has largely completed her physical development before the age of 14 or 15. By referring to the charts of my New Haven tests,[1] those for muscle sense, force of suggestion, voluntary motor ability, fatigue, lung capacity, reaction, reaction with

[1] Researches on the Mental and Physical Development of School Children, *Stud. Yale Psych. Lab.* Vol. II, p. 40.

discrimination and choice, and time memory, all give clear indications of a marked change in the development at this period. It is interesting to notice the relation between some of the tests taken in Iowa City and the corresponding tests taken upon the New Haven school children. In lung capacity the Iowa children excel those of New Haven at all ages. In height they are about on an equality at age 6 but at age 17, which was the limit in age for the New Haven tests, the boys of Iowa are 2.6 centimeters and the girls 2.2 centimeters taller than those of New Haven. This would seem to be due to the environment of the two classes of children, for in weight the difference is even more marked. At age 6, New Haven boys and girls are 2.0 and 4.2 pounds the heavier respectively than the Iowa City children, but by the time age 17 is reached the Iowa boys and girls are the heavier by 12.3 and 2.6 pounds respectively. It must be noted in this connection that a number of the data in this series of tests are taken from West Liberty children, a good proportion of whom are drawn from the near country regions. Boston and Milwaukee school children are still lighter and shorter than either New Haven or Iowa City children.

Several things are worthy of notice in comparing the various charts giving the relation to mental ability. As remarked above, my data show no such relation between weight and height and mental abililty as has been claimed, but give a negative result, and if any positive result can be stated at all, it would be that the taller and heavier the children the duller they are, instead of the opposite. The marked differences occur between ages 10 and 14. By referring to the charts for graded weight, lung capacity, and wrist lift, it will be seen that they all follow approximately the same law.

On the Effects of Loss of Sleep.[1]

BY

G. T. W. PATRICK AND J. ALLEN GILBERT.

The object of the following experiments was to determine some of the physiological and mental effects of enforced abstinence from sleep. In an address before the International Medical Congress at Rome in 1894, M. de Manacéine reported some experiments upon young dogs on the effects of absolute insomnia. The animals were kept from sleeping, and died at the end of the fourth or fifth day.[2] So far as is known to the present writers, no experiments upon human subjects have hitherto been made on enforced insomnia for psychological purposes. The plan of our experiments was as follows: It was proposed to keep the subjects awake continuously for about 90 hours, to make a series of physiological and psychological tests upon them at intervals of 6 hours in respect to reaction-time, discrimination-time, motor ability, memory, attention, etc.; to observe secondly, the general effects of insomnia; and finally to observe the depth, character and amount of sleep following the period of waking. This plan was successfully carried out with three subjects, the depth of sleep being ascertained, however, in the case of only one. The subjects were in each case constantly attended by either one or two watchers. They took their regular meals at 7 A. M., 12:30 P. M., and 6 P. M., the food being normal in character and amount. In addition they ate a very light lunch at 12:30 A. M. The days

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[1] Reprinted from the *Psychological Review*, Vol. III, No. 5.
[2] *Archiv. Ital. Biol.* XXI, 2.

were spent in occupations conforming as nearly as possible to the usual daily work of the subject. The nights were spent at first in reading or playing light games, and toward the end of the experiments in any way best adapted to keep the subjects awake, such as walking, working upon apparatus, or playing active games. Each set of experiments, however, took nearly two hours, so that this occupation consumed almost one-third of the time both day and night.

We give first a general account of the subjects and experiments. The first subject, J. A. G., is a young man of 28 years, assistant professor in the University. He is unmarried, of perfect health, of nervous temperament, of very great vitality and activity. He is accustomed to about 8 hours of sound sleep from 10 P. M. to 6 A. M. He awoke at his usual time Wednesday morning, November 27, and remained awake until 12 o'clock Saturday night. The second night he did not feel well and suffered severely from sleepiness. The third night he suffered less. The fourth day and the evening following he felt well and was able to pass his time in his usual occupations. During the last 50 hours, however, he had to be watched closely, and could not be allowed to sit down unoccupied, as he showed a tendency to fall asleep immediately, his own will to keep awake being of no avail. The daily rhythm was well marked. During the afternoon and evening the subject was less troubled with sleepiness. The sleepy period was from midnight until noon, of which the worst part was about dawn.

The most marked effect of the abstinence from sleep with this subject was the presence of hallucinations of sight. These were persistent after the second night. The subject complained that the floor was covered with a greasy-looking, molecular layer of rapidly moving or oscillating particles. Often this layer was a foot above the floor and parallel with it and caused the subject trouble in walking, as he would try to step up on it. Later the air was full of these dancing particles which developed into swarms of little bodies like gnats, but colored red, purple, or black. The subject would climb

upon a chair to brush them from about the gas jet or stealthily try to touch an imaginary fly on the table with his finger. These phenomena did not move with movements of the eye and appeared to be true hallucinations, centrally caused, but due no doubt to the long and unusual strain put upon the eyes. Meanwhile the subject's sharpness of vision was not impaired. At no other time has he had hallucinations of sight and they entirely disappeared after sleep.

The period of 90 hours being completed at 12 o'clock Saturday night, the subject was allowed to go to sleep, which he did immediately. He was awakened at intervals of one hour to ascertain the depth of sleep, but fell asleep again at once after each awakening, and slept until half past ten Sunday morning. He awoke then spontaneously, wholly refreshed, felt quite as well as ever, and did not feel sleepy the following evening. He slept, however, two hours later than usual Monday morning.

The special tests made upon this subject, 14 in number, are shown with the results in Table I. They were all repeated every 6 hours throughout the whole period, and repeated again finally after the subject had slept. The results of the latter tests are shown in the last column. In reaction-time and discrimination-time, the effects of practice were eliminated as far as possible by preparatory training preliminary to the experiment. A few words of explanation of methods and apparatus are necessary. The pulse was taken at the beginning of each set of tests and then again at the end immediately after the subject was fatigued by tapping with the forefinger as rapidly as possible for 60 seconds. The subject was weighed the same time after each meal and in the same clothing. Grip was taken with an ordinary hand dynamometer. Pull was taken with the same instrument, the subject using the second finger of each hand.

For reaction-time the stimulus was a telephone click, with signal, the reaction being the release of a key, the subject being in the dark room, away from the recording drum Each reaction-time given represents the mean value of from 10 to 15 reactions. For discrimination a modification of the same appa-

J. A. G.

TABLE I.

	November 27.			November 28.				November 29.				November 30.				Dec. 1. After Sleep.
	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M	3 P. M.	9 P. M.	12 M.
1. Pulse	88	89	68	62	81	72	74	74	68	65	63	63	61	72	61	77
2. Temperature (Centigrade)	36.72	36.39	36.17	35.78	36.56	36.67	36.56	35.67	36.44	36.56	36.11	36.28	36.00	36.50	36.39	36.17
3. Weight (Kilograms)	67.70	67.75	68.30	67.78	68.19	68.04	68.52	68.83	68.27	67.99	68.35	68.60	68.41	68.13	68.47	67.39
4. Grip (Kilograms)	48.08	46.95	51.94	47.86	47.17	44.45	40.83	——	44.91	48.08	47.17	45.36	43.99	49.67	43.77	50.35
5. Pull (Kilograms)	27.22	27.67	28.12	26.31	26.76	25.86	22.68	22.68	26.31	25.86	24.95	23.59	22.68	26.99	23.13	27.67
6. Reaction-time. Mean (Sec.)	.122	.132	.129	.149	.133	.129	.139	.143	.146	.130	.144	.146	.139	.165	.148	.128
Mean variation	9	26	28	20	10	16	24	25	21	10	50	21	31	26	20	13
7. Reaction-time with Discrimination and Choice. Mean	.258	.240	.242	.253	.225	.215	.216	.271	.207	.210	.213	.213	.206	.201	.158	.205
Mean Var.	50	56	51	48	38	32	43	67	63	63	40	65	62	36	43	52
8 Sensibility to Pain. Lower threshold	——	3250	3000	3100	2750	3100	2650	2800	3150	2800	2750	2850	3300	3150	3250	3200
Upper threshold	——	4450	4350	4550	4050	4650	4450	4300	4600	4250	4200	4300	4550	4850	4400	4800
9. Acuteness of Vision (Cm.)	137.2	132.1	139.7	134.6	142.2	156.2	150.5	120.6	137.2	143.5	137.2	152.4	148.6	156.8	171.4	125.7
10. Memory (Sec.)	——	540	260	159	290	330	200	105	240	70	262	290	123	190	545	125.
11. Addition of Figures	——	228	254	248	238	249	223	215	205	216	196	210	200	250	224	277.
12. Voluntary Motor Ability	42.2	42.2	40.1	39.0	40.0	41.2	38.6	35.5	39.5	39.0	35.0	38.9	41.0	39.0	39.7	41.3
13. Fatigue. Per cent. of loss	24.1	24.6	22.6	20.5	18.0	24.0	13.7	12.1	17.0	13.9	11.4	20.6	17.6	17.9	13.6	17.7
14 Pulse after Fatigue	89	81	92	82	75	76	58	59	62	62	54	58	63	59	52	84

ratus was used, the subject reacting only to the loud stimulus. Sensibility to pain was tested by a specially prepared algometer, arranged to bring any desired pressure upon the middle of the fingernail of the first finger, the finger being inserted between two horizontal bars, the one pressing upon the fingernail being a very dull wooden knife edge. The figures record the pressure in grams, the lower threshold representing the first feeling of pain, the upper threshold the point at which the pain could no longer be endured. Acuteness of vision was tested in the dark room by finding the greatest distance at which the subject could read a section of a page from Wundt's *Studien* by the light of one standard candle at a distance of 25 cm. The memory test consisted in committing to memory 10 of the Ebbinghaus nonsense syllables. These were used in the ordinary way, but we consider this test of very slight value, for it is impossible not to learn these lists by association, and impossible to get different lists which offer equal ease or difficulty in association. The effects of loss of sleep upon attention and association we attempted also to ascertain by determining the greatest number of figures in prepared columns that could be added in three minutes. Voluntary motor ability was tested by having the subject tap with the forefinger as rapidly as possible upon a key for 5 seconds, using the recording drum and graphic chronometer. He then continued tapping for 60 seconds to fatigue the muscles. The number of taps during the last 5 seconds was then recorded. In the table is given first the number of taps in the first 5 seconds, then the percentage of loss in the last 5 seconds due to fatigue. The results of the special tests may best be studied from the table. Attention is called, however, especially to the following. The steady increase in the subject's weight during the experiment and the sudden decrease in weight after sleep are noteworthy, and apparently not to be accounted for by accidental circumstances. His average weight during the last 24 hours was 18 ounces greater than the average during the first 24 hours, and at 9 o'clock Saturday night the subject weighed 27 ounces more than at 9 o'clock Wednesday morning. During the 10½ hours'

sleep, however, which followed the experiment, the subject lost 38 ounces, being 11 ounces more than he had gained during the experiment. In the tests with the dynamometer the subject lost slightly and gradually in strength of both grip and pull, regaining all after sleep. On Saturday afternoon, however, the subject made what appeared to be a spurt, in view, perhaps, of the approaching end, and gripped and pulled nearly as much as at the beginning. The reaction-time beginning with 122σ increased somewhat regularly, reaching its maximum, 165σ Saturday afternoon, after 81 hours without sleep, and dropped back to the normal immediately after sleep. The discrimination-time appears to decrease, but as it does not increase after sleep the result cannot in this case be attributed to loss of sleep. The acuteness of vision uniformly *increased* throughout the experiment, falling below the normal after sleep. The slight retardation in the increase in the second night corresponds with the period of slight sickness at that time. There is a significant decrease in voluntary motor ability. The decrease in this subject's pulse-beat after fatigue by tapping is abnormal and apparently a result of loss of sleep.

The above experiment upon J. A. G. was regarded as somewhat preliminary. It was, therefore, decided to repeat the experiment upon two other subjects, making such modifications in the special tests and apparatus as seemed to be desirable. The second subject, A. G. S., was a young man of 27 years, instructor in the University, unmarried, quiet and of excellent health. The third subject, G. N. B., was a young man of 24 years, instructor in the University, unmarried, of German parentage, stout and perfectly healthy. At the time of the experiment, A. G. S. was accustomed to 9 hours of sound and regular sleep; G. N. B. to 8 hours. These two subjects entered upon their sleep fast at 7 o'clock, Tuesday morning, March 17. Ninety hours was again the period determined upon. On Friday night, March 20, at 11.15, the last set of experiments being completed, they were allowed to retire, so that their waking period was actually 88¼ hours. In the case of these two subjects there was no illness, no hallu-

TABLE II.

	March 17.			March 18.				March 19.				March 20.				Mar. 21 After Sleep.
	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	1 P. M.
1. Pulse	74	68	75	61	73	73	72	71	79	62	67	61	74	68	63	76
2. Temperature (Centigrade)	37.11	36.39	36.78	37.11	37.00	37.22	36.89	——	36.89	36.44	36.56	36.33	37.06	36.67	35.33	37.22
3. Weight (Kilograms)	67.02	67.47	67.47	67.24	66.68	67.24	67.13	66.68	67.02	67.36	67.47	67.59	67.02	67.36	67.59	67.24
4. Grip (Kilograms)	33.56	39.92	30.39	33.11	33.56	29.03	24.04	24.04	28.12	29.48	26.31	26.76	29.03	30.39	27.22	33.56
5. Pull (Kilograms)	155.58	163.30	140.62	117.94	150.60	113.40	127.00	81.65	107.05	89.36	88.45	49.44	49.44	95.26	92.99	131.54
6. Reaction-time. Mean	.121	.134	.138	.134	.141	.138	.143	.154	.147	.150	.141	.146	.143	.148	.193	.160
Mean variation	0.6	1.5	0.8	0.9	2.2	1.2	7.1	1.5	1.7	1.9	2.4	1.5	1.9	2.5	4.0	2.9
7. Reaction-time with Discrimination and Choice. Mean	.158	.200	.310	.175	.202	.201	.182	.162	.188	.280	.189	.170	.222	.176	.311	.231
Mean var.	5.9	4.2	7.4	4.1	3.5	4.5	2.9	3.6	4.1	8.3	4.7	3.9	5.3	4.3	8.0	6.0
8. Acuteness of Vision (Cm.)	——	103.8	103.8	113.6	122.3	112.8	96.1	105.1	115.4	116.6	119.2	109.0	119.7	118.0	123.3	119.7
9. Discrimination of Sound	8.0	12.5	10.0	11.2	11.6	13.0	12.5	31.0	12.5	22.0	21.0	31.0	23.0	18.7	18.0	16.5
10. Memory (Sec.)	115	110	112	143	129	145	102	159	120	152	217	202	139	100	570	88
11. Addition of Figures	——	85	119	118	105	103	130	192	111	108	185	610	113	190	345	109
12. Naming of Letters	165	160	155	154	162	155	134	113	154	144	127	91	147	135	117	171
13. Voluntary Motor Ability	38	36	33	37	41	36	30	34	36	36	37	28	39	38	34	42
14. **Fatigue.** Per cent. of Loss	29.0	13.9	15.1	13.5	29.3	16.6	13.3	26.5	19.4	25.0	21.7	0.00	20.5	23.7	26.5	21.4
15. Pulse after Fatigue	80	69	69	66	79	71	77	65	72	75	64	62	70	64	61	83

TABLE III.

	March 17.			March 18.				March 19.				March 20.				Mar. 21 After Sleep.
	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	3 A. M.	9 A. M.	3 P. M.	9 P. M.	4 P. M.
1. Pulse	63	64	63	68	68	67	67	69	70	62	64	68	74	65	73	84
2. Temperature (Centigrade)	36.22	36.44	36.33	37.17	36.78	37.22	36.56	36.67	36.33	36.61	36.56	36.89	37.06	35.78	36.56	37.22
3. Weight (Kilograms)	68.49	69.29	69.29	69.17	69.51	69.74	69.85	69.99	69.85	69.99	70.08	70.08	69.40	69.74	69.85	69.29
4. Grip (Kilograms)	42.64	34.47	38.10	33.11	39.36	43.09	37.65	34.01	37.19	37.65	42.64	43.09	44.45	47.63	44.00	41.73
5. Pull (Kilograms)	118.84	129.28	146.15	138.35	125.19	117.94	106.6	111.13	120.20	113.40	113.40	113.40	113.40	111.13	95.26	117.94
6. Reaction-time. Mean	.145	.148	.157	.130	.142	.143	.134	.187	.136	.137	.141	.123	.139	.141	.142	.124
Mean variation	1.8	1.3	1.4	0.8	1.1	1.7	1.8	2.9	3.7	1.0	1.7	1.8	1.1	2.9	3.2	1.6
7. Reaction-time with Discrimination and Choice. Mean	.167	.170	.200	.140	.185	.177	.214	.170	.178	.147	.158	.133	.153	.143	.175	.166
Mean var.	3.6	7.2	5.6	1.4	3.7	1.6	5.5	4.6	6.8	1.8	2.7	3.7	2.3	1.2	2.3	4.0
8. Acuteness of Vision	——	110.3	115.4	141.0	132.1	134.6	127.4	129.5	134.6	119.2	137.2	126.9	130.9	128.7	135.9	134.6
9. Discrimination of Sound	12.8	20.0	12.5	24.8	14.0	15.0	20.5	17.5	24.5	30.0	23.0	20.0	22.5	21.0	32.0	21.5
10 Memory	170	133	128	206	170	135	273	143	112	353	169	201	820+	645	900+	106
11. Addition of Figures	——	120	125	141	135	122	140	141	135	120	118	115	118	123	130	109
12. Naming of Letters	177	180	169	165	183	163	158	158	156	165	154	156	157	148	117	188
13. Voluntary Motor Ability	41	37	38	39	41	42	34	39	39	39	40	44	42	42	40	40
14. Fatigue. Per cent. of Loss	19.5	16.2	26.3	28.2	29.3	28.6	14.7	28.2	25.6	33.3	25.0	34.1	26.2	26.2	35.0	22.5
15. Pulse after Fatigue	70	69	60	69	79	63	64	69	79	64	55	64	77	69	70	96

cinations of sight, and no serious suffering or discomfort. A. G. S. became very sleepy during the last 24 hours and had to be watched constantly. On Friday, at 9 P. M., after a brisk walk in the cool air, his temperature sank to 35.3° Cent. (95.6° F.), but in 15 minutes rose to 36.3° Cent. (97.3° F.) Of the three subjects he was the only one who apparently could not have prolonged the experiment beyond the period of 90 hours without danger. G. N. B. had less trouble in keeping awake and showed outwardly but slight effects of the abstinence from sleep. Both subjects slept immediately upon retiring at 11:15 P. M., Friday. They both slept uninterruptedly until 10:30 A. M. Saturday. They both awoke then for a few moments and slept again, A. G. S. until 11:15 A. M., G. N. B. until 2:40 P. M. They both felt wholly refreshed upon awaking, required no further extra sleep, and felt no ill effects from the experiment.

The special tests made upon these two subjects are shown with the results in Table II. and Table III., and exhibited, in part, in graphic form in the subjoined curves. They were as before repeated every 6 hours. To eliminate, as far as possible, the effects of practice, the tests were begun two or three days before the beginning of the sleep fast. The first three sets of results in the tables, being taken the first day before any loss of sleep, should represent the normal reaction of the subject. These, taken together with the results of the tests made after awaking shown in the last column of the tables, make a fairly adequate standard for comparison with the results obtained during the sleep fast. The tests in respect to pulse, temperature, weight, grip, reaction-time, discrimination-time, sharpness of vision, voluntary motor ability, and fatigue, were the same as described above for the first subject. The strength of pull was taken with an ordinary lift dynamometer, the subject, standing upon a small platform with bent knees and straightened back, lifting his utmost by means of two handles connected by ropes with a large spring balance. In the memory test, the nonsense syllables were discarded and 18 figures substituted. Eighteen small squares

of cardboard were provided upon which were printed the 9 figures, each figure thus appearing twice. For each experiment a random order of these figures was made, and then modified, if necessary, to prevent adjacence of same figure and suggestive combinations. The subject, timed with a stop watch, committed to memory the list, the watch being stopped when the subject announced his readiness to recite the list. Each experiment consisted in committing to memory three such lists. The tables show in seconds the average of these three trials in each case. No. 11 was a test in adding numbers. The sheets of figures used by Miss Holmes in studying fatigue in school children and described in the Pedagogical Seminary, Vol. III, No. 2, were used. The subject was required to add each set of 40 figures by twos, setting down the results. He then added the results and then added the original figures in a different order. Any variation recorded in the two results indicated errors. The tables give the time required for the whole process. Test No. 12 was designed to determine the subject's facility in seeing and naming letters. A page from *The Psychological Review* was used; the subject reading the lines backward merely named the letters as fast as possible. The tables record the number of letters, average of two trials, named in one minute. Test No. 9 was designed to show the acuteness of hearing by discrimination of the intensity of two sounds. The sounds were vibrations of a tuning fork heard in a telephone in the silent room, the intensity being varied by a resistance board, only one telephone being used. The results in the tables have only relative value, indicating the number of divisions upon the resistance board by which the resistance had to be increased to enable the subject to detect the difference in the intensity of the sounds.

We may call special attention to a few of the results. In both subjects we again observe an increase in weight throughout the experiment with decrease after sleep. But with these subjects the decrease is less than the increase. In strength of lift both subjects lose quite regularly and seriously, but regain nearly all after sleep. In the memory tests, the results

are very marked, especially with G. N. B. His average time in normal condition for committing the 18 figures was 134 seconds. No remarkable increase in this time was observed until the expiration of 72 hours. At 9 A. M., Friday, the subject required 960 seconds to commit the first set of figures and failed entirely to commit the third set, working at it for *20 minutes*. At 9 P. M. he could not commit the figures, and having made no progress after 15 minutes he desisted. The attention could not be held upon the work. A kind of mental lapse would constantly undo the work done. With both subjects an energetic 'waking up' by means of brisk walking and fresh air was often necessary during the latter time in order to address themselves to these mental tasks. After sleep, A. G. S. easily committed the figures in 88 seconds, and G. N. B. in 106 seconds, this being in both cases the shortest time in which the work was done. In respect to the number of letters named in one minute, there is with both subjects a steady decrease with the progress of the insomnia, with immediate return to the normal after sleep. In adding numbers similar results appear in a marked form in the case of A. G. S., but with G. N. B. adding time was affected but slightly. Reaction-time increases with A. G. S., as with J. A. G., but the reaction-time of G. N. B. is not lengthened. In respect to reaction with discrimination and choice the results are irregular and unsatisfactory. There is an irregular increase with A. G. S., but an actual shortening of time with the other two subjects.

Attention should be called to the length of sleep following the sleep fast and its relation to the whole amount of sleep lost. A. G. S. found it necessary to make up but 16 % of the lost sleep, as measured by time; J. A. G. 25 %; G. N. B. 35.3 %. As restoration was in each case apparently complete, explanation must be sought in one of two hypotheses or in both. The first is that, owing to the greater 'depth' of sleep after the sleep fast, the anabolism accompanying restoration was more rapid. The second is that the partial restoration which normally accompanies the waking period was, in the

case of this long waking, greater than usual; that the subjects, in other words, although apparently awake and, indeed, as wide awake as they could be kept, were nevertheless at times partially asleep. There are reasons to believe that the results depend upon both of these causes. Our subjects well illustrated the fact that sleep is a matter of degree. All that could be done both by objective diligence and subjective effort to keep the subjects wide awake was done. If the subject, contrary to his own intention, closed his eyes, although he immediately opened them in response to his watcher's command, still there was time for a short and perhaps refreshing 'nap.' Again, one of our subjects, who was kept jogging about the streets during a sleepy period at 5 A. M., afterwards could remember little about the walk. Another subject, standing with eyes open, reflectively gazing at a piece of apparatus upon which there were some pieces of rope, suddenly reported that he had had a dream about a man being hung. With our first subject we undertook to test the delicacy of the muscle sense by means of lifting weights. These were small tin pails loaded with graded weights and lifted by a detachable handle. Lifting these pails was found to be very monotonous and sleepy work. The subject was not permitted to let his attention wander. and yet he reported at least four dreams. For instance, he lifted two pails, carefully judged their relative weight, and as he set the second one down, instead of saying that No. 1 or No. 2 was the heavier, he said 'trimmings,' evidently having fallen asleep as he was lifting or setting down the pails and dreamed that they contained trimmings. It must be understood that these dreams were instantaneous and the subject as wide awake as he could be kept, but these facts reveal a cerebral condition related to sleep. This hypothesis alone, however, would not seem to account fully for the small proportion of sleep made up. And, indeed, a study of our special tests shows that restoration took place chiefly during the profound sleep following the sleep fast, and took place rapidly. That this sleep was actually more profound and that the profound part of it was longer

than usual was shown by our experiments in depth of sleep in the case of J. A. G. reported below.

The depth of normal sleep for the consecutive hours of the night has been studied by Michelsen and by Kohlschütter, and the results presented in the so-called sleep curves. The depth of sleep was determined by these observers by the intensity of sound necessary to awaken the sleeper. Their results show the greatest depth of sleep at the end of the first hour. After the first hour the curve drops abruptly and rapidly. Already at the end of the second hour sleep is light and continues slowly to become lighter until morning. In the case of our first subject, J. A. G., we attempted to ascertain the relative depth of sleep for the consecutive hours of the profound sleep following the sleep fast, for the sake of comparing our results with the normal sleep curve. As a sound stimulus would not be practicable, for the reason that, the experiments all being made in the same period of sleep, the sleeper would soon become accustomed to it, we substituted a pain stimulus. An electric garter, to which the subject had become accustomed by wearing it for some nights preceding the sleep fast, was attached to the sleeper's ankle and connected with an induction coil in an adjoining room, and so arranged that the current could be closed for a constant time, viz., .334 sec., by means of a pendulum, and that the strength of the current could be varied by means of a resistance tube. It was agreed that the sleeper should announce his awaking by means of an electric button at his bedside. The current was turned on at intervals of one hour. Unfortunately the least resistance that could be arranged with the resistance tube failed to awaken the sleeper at the first three periods, so that it was necessary to cut out the tube and the pendulum and apply the direct current and measure it roughly by the time the circuit had to be closed. Our results, therefore, lack the exactness necessary for the construction of a curve or table, but still show plainly the relative depth of sleep for the consecutive hours. The deepest sleep was found at the end of the second hour, when the subject could not be aroused sufficiently to ring the bell,

but responded by a cry of pain. The next deepest sleep was found at the end of the first hour and the next at the third hour. The current used at these three times was one which it was altogether out of the question for the subject to endure when awake. At the end of the second hour, just after the experiment, we entered the sleeper's room and attempted to awaken him by speaking to him in a loud voice without avail. At the fourth hour the sleep was less deep, and continued to become lighter regularly until awaking, but the decrease in depth was very much less rapid than in the normal sleep curves reported above. At 10 A. M. a very slight current awakened the sleeper, and at 10:30 he awoke spontaneously as stated.

The tendency of our subjects to have short semi-waking dreams suggested to us that in enforced insomnia there would be offered a good opportunity for a study of dreams. This of course was incompatible with our purpose, but in the cases of A. G. S. and G. N. B., at the end of the sleep fast and before allowing the subjects to retire, we undertook a few experiments in dreams. We allowed the subjects to sit with head supported behind, and to sleep for periods of 30 seconds, one minute, three minutes, etc., then awakening them and asking for their dreams. No dreams were obtained in any case. If the period was less than one minute the subject sometimes had a hazy memory of something like a dream which could not be put into words. If the sleep was longer it was apparently profound and dreamless. These rough experiments confirm, of course, the generally accepted opinion that dreams are the product of light sleep, representing indeed the reinstatement of consciousness after the early and profound sleep.

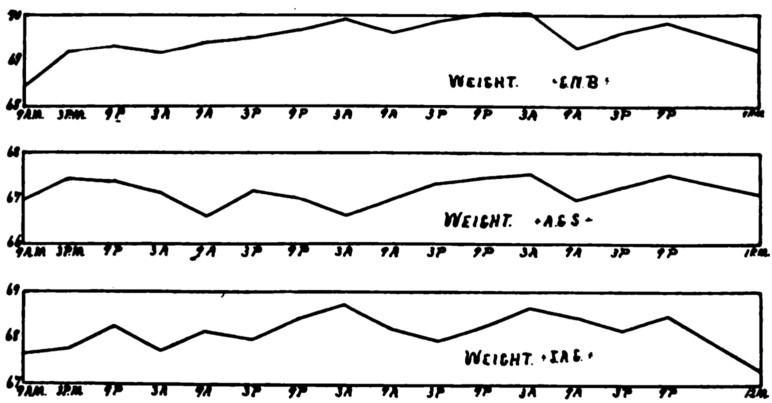
Through the kindness of Dr. E. W. Rockwood, of the University, a chemical analysis of the urine was made throughout the experiments in the case of each of the subjects. The object of the analysis was to determine the influence of continued waking upon the relative amounts of nitrogen and phosphoric acid respectively excreted. The results are fully exhibited in Table IV, as compiled by Dr. Rockwood. Considered in relation to the fact that each subject increased in

TABLE IV.

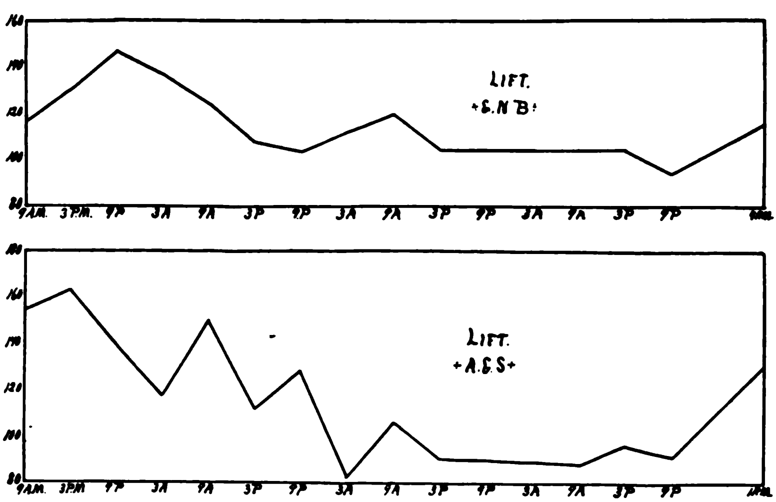
J. A. G.	2d day before experiment.	1st day before experiment.	1st day of experiment.	2d day of experiment.	3d day of experiment.	4th day of experiment.	4th day of experiment. (Sleep.)	1st day after experiment.	2d day after experiment.
Hours................			24	24	24	14	11¾	24	
Total amount urine (ccm.)			1475	1370	1270	805	400	950	
Grams N. per hour.....			0.901	0.929	0.667	0.723	0.490	0.723	
Grams P_2O_5 per hour...			0.1327	0.1438	0.1105	0.1304	0.0564	0.0888	
Relation P_2O_5 to N.....			1:6.8	1:6.5	1:6.0	1:5.5	1:8.7	1:8.1	
A. G. S.									
Hours................	38		24	24	24	13½	12¾	24	24
Total amount urine (ccm.)	1308		1510	1700	1420	750	525	1000	1240
Grams N. per hour.....	0.655		0.661	0.628	0.745	0.661	0.414	0.6175	0.761
Grams P_2O_5 per hour...	0.0765		0.0708	0.0791	0.1011	0.1000	0.0674	0.0907	0.1023
Relation P_2O_5 to N.....	1:8.6		1:9.3	1:7.9	1:7.4	1:6.6	1:6.1	1:6 8	1:7.5
G. N. B.									
Hours................	24½		24	24	23	13½	16½	24½	24
Total amount urine (ccm.)	920		1240	1205	1730	650	365	705	705
Grams N. per hour.....	0.4853		0.7094	0 6270	0.6123	0.5195	0.3390	0.5020	0.4765
Grams P_2O_5 per hour...	0.0574		0 0802	0.0931	0.0826	0.0815	0.0435	0 0616	0.0613
Relation P_2O_5 to N.	1:8.5		1:8.8	1:6.7	1:7.4	1:6.4	1:7.8	1:8.1	1:7.8

weight during the insomnia, the results are significant. They show not merely that there was an increase in the excretion of both nitrogen and phosphoric acid during the period of insomnia, but that relatively more phosphoric acid was excreted than nitrogen. A certain amount of support is thus given to the theory of a special connection between mental activity and the katabolism of the phosphorized bodies of the nervous system.

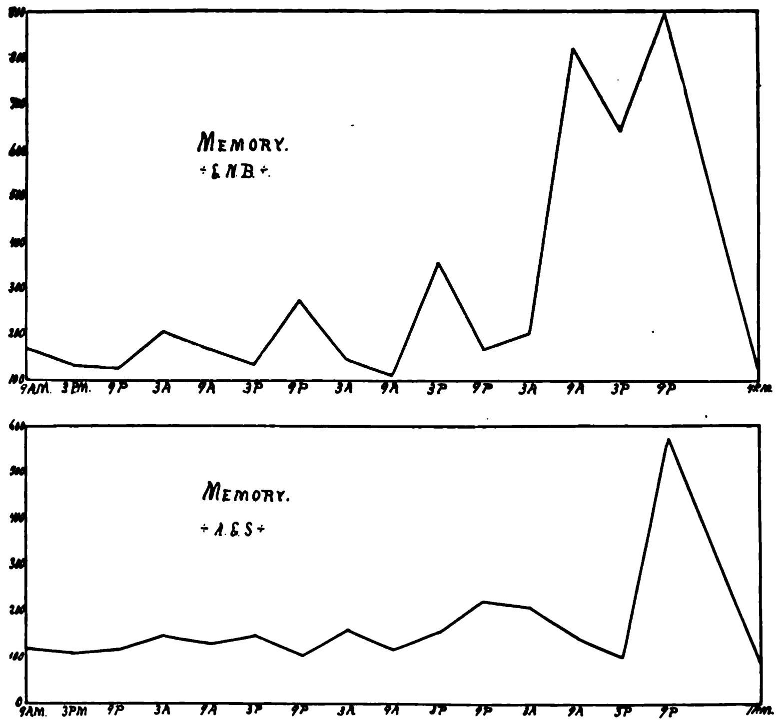
In the case of the subjects A. G. S. and G. N. B. the changes in the amount of nitrogen and also of phosphoric acid, and the ratio of these to each other are exhibited in graphic form in the accompanying charts.



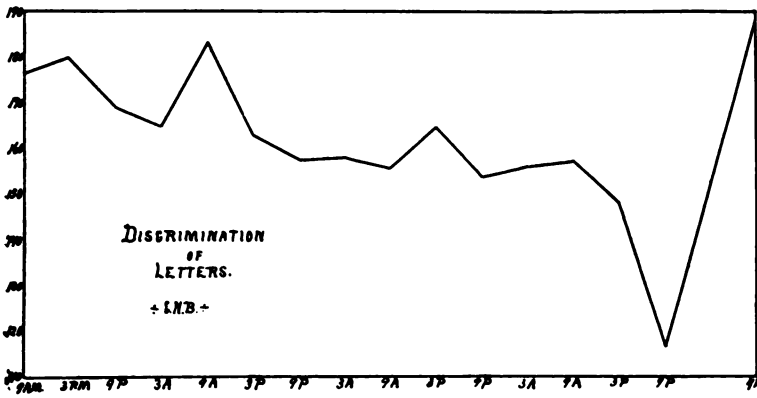
EXPLANATION: The ordinates show the weight of the subject in kilograms. The abscissae show the progress of the hours during the waking period, the last interval, however, representing the period of sleep following.



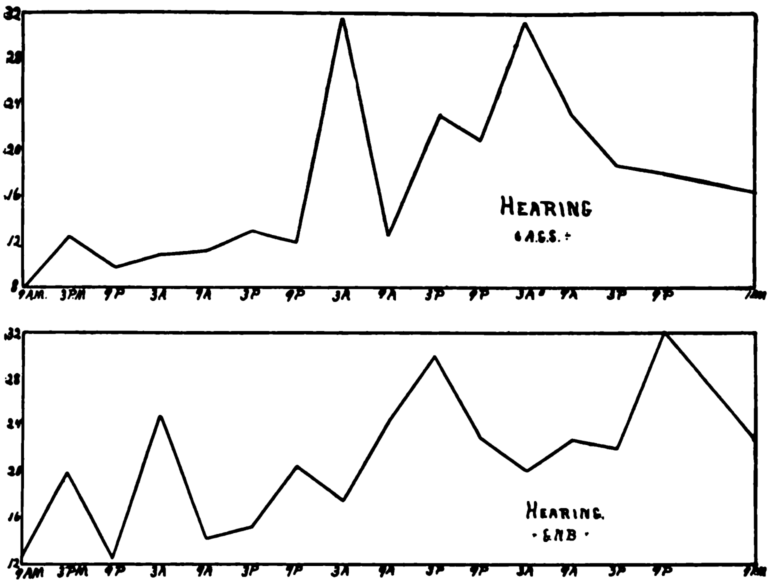
EXPLANATION: The ordinates show the strength of lift in kilograms. The abscissae show the progress of the hours as explained above.



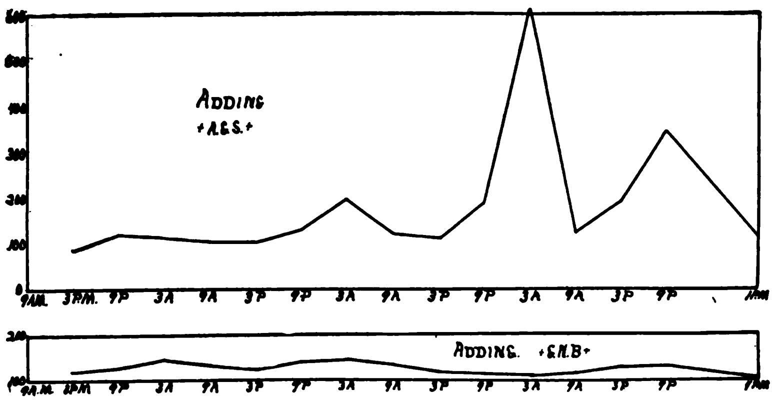
EXPLANATION: The ordinates show the number of seconds required to commit to memory 18 digits. The abscissae show the progress of the hours as explained on page 55.



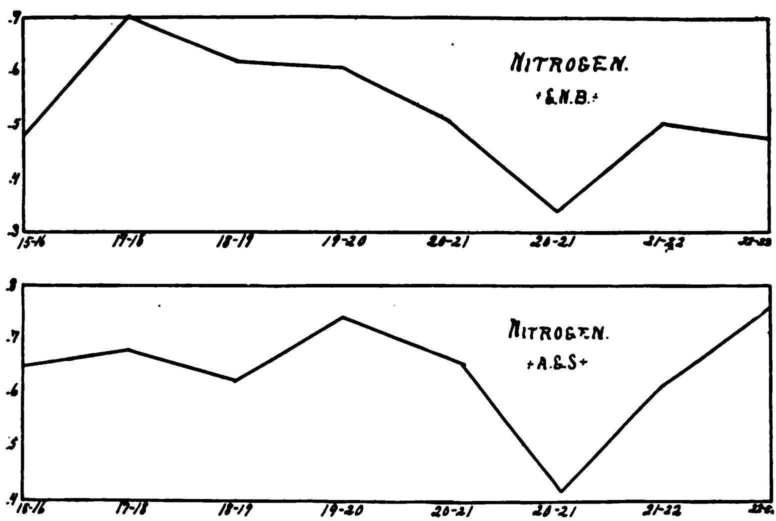
EXPLANATION: The ordinates show the number of letters named in one minute. The abscissae show the progress of the hours as explained on page 55.



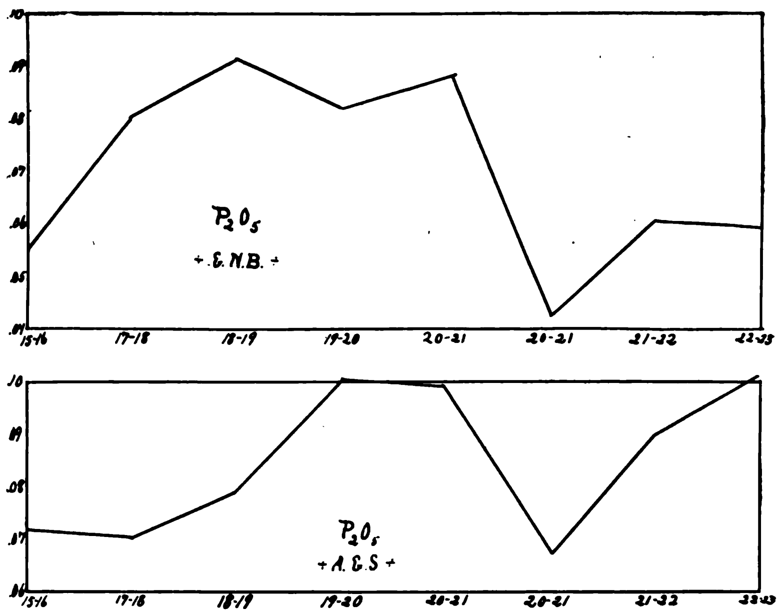
EXPLANATION: The ordinates show the relative intensities of sound required for discrimination. The abscissae show the progress of the hours as explained on page 55.



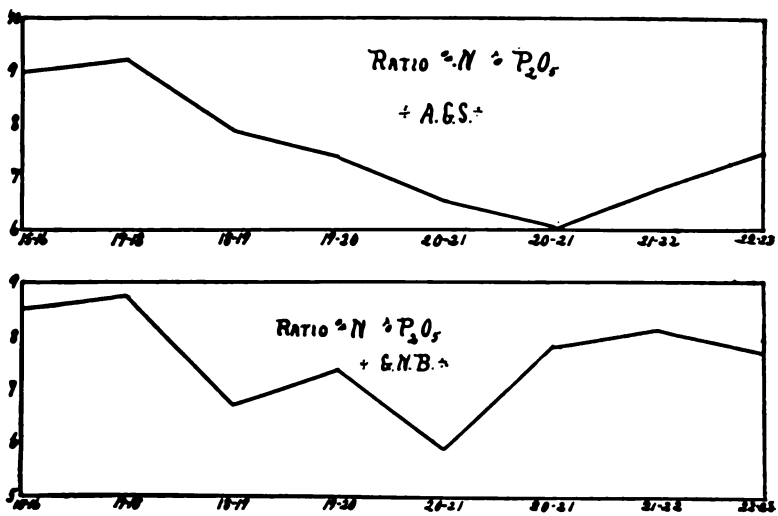
EXPLANATION: The ordinates show the number of seconds required to add a given number of figures. The abscissae show the progress of the hours as explained on page 55.



EXPLANATION: The ordinates show the number of grams of nitrogen excreted per hour. The abscissae show the days upon which the tests were made, the sleep fast ending at the second point marked 20–21.



Explanation: The ordinates show the number of grams of phosphoric acid excreted per hour. The abscissae show the days upon which the tests were made, the sleep fast ending at the second point marked 20–21.



EXPLANATION: The ordinates show the ratio of nitrogen to phosphoric acid excreted. The abscissae show the days upon which the tests were made, the sleep fast ending at the second point marked 20–21.

THE EFFECT OF PRACTICE IN REACTION AND DISCRIMINATION FOR SOUND UPON THE TIME OF REACTION AND DISCRIMINATION FOR OTHER FORMS OF STIMULI.

BY

J. ALLEN GILBERT AND G. CUTLER FRACKER.

IT is a well known fact that practice reduces the length of both reaction time and discrimination or discernment time.

The aim of the present investigation was to determine what effect practice in reaction and discrimination with stimuli in one sense has on the same processes in other senses, the latter not having been practiced at all. For convenience the sense of hearing was used in practice.

GENERAL METHODS.

The time of reaction and of reaction with discrimination and choice was first taken with each of the three subjects for each of the following stimuli: Sound, light, pressure, and electricity. Each day thereafter for 12 days, two of the subjects were practiced in reacting and discriminating with sound stimulus. The third subject practiced 11 days. After that time the series was taken again as at the beginning, in order to determine whether practice in discrimination and reaction for sound would also reduce the time of the same processes for light, pressure, and electrical stimulation, without practice in the latter. The experiments were taken upon J. A. G., G. C. F., and J. C. P. During the 12 days' practice, the first two subjects practiced to reduce both discrimination and reaction

time to sound. The last subject, who is the present champion sprinter in the University, was practiced in reaction to hearing alone, thus giving opportunity in the problem to determine what effect practice in reaction alone has on reaction and discrimination of all four kinds named above. The full series of reactions and discriminations in the different series was taken, however, on all three subjects before and after the interval of practice. All three subjects are men in excellent health, aged 30, 29, and 23 respectively. The series of tests was begun April 24, 1897, and the practice continued each day thereafter, excluding Sundays and two other days, when the work was unavoidably interfered with. The two series of tests before and after the practice were taken at 4:30 P. M. The hour for practice was the same with the exception of the two Saturdays, on which the hour was in the morning. The hour for J. C. P. was 3 P. M. throughout.

The reaction with discrimination and choice was always taken previously to the simple reaction. This order reduces the liability to react to every stimulus, both weak and strong, where discrimination is desired, with reaction to the strong stimulus alone. J. C. P. with even this precaution found it difficult to withhold reaction to the weak sound inasmuch as he was accustomed to reacting to every stimulus.

In all the tests the warning used was a "snapper-sounder," struck two seconds before the stimulus for reaction.

APPARATUS.

The method used in taking the time was the graphic spark method,[1] recording the time to the thousandth of a second. In taking the reaction and discrimination according to the different kinds of stimuli, appropriate apparatus was thrown into the primary circuit of the spark coil, as will be described below.

For Hearing.

For reaction and discrimination for sound, the telephone click was used and discrimination was for differences in inten-

[1] *Studies from the Yale Psych. Lab.* Vol. I. p. 3.

sity. To produce the two different intensities of sound which were to be discriminated, the clicks in the telephone receiver were made by throwing into the circuit of the receiver a slide inductorium which made it possible to properly adjust the relative intensities of the two sounds by sliding the secondary coil over the primary coil farther for the loud sound to be given in the telephone receiver, connected with the secondary poles, than for the weak sound. Having previously determined the two positions for the secondary coil of the inductorium at which the two sounds were easily distinguished in intensity, they were kept constant throughout the experiments, introducing the strong or weak sound at will by shifting the secondary coil to the two different positions. In discriminating between the two intensities the subject was required to react when the loud sound was heard in the telephone receiver and not to react when the weak sound was heard. Though the subject was in another room, it was always possible to tell whether he had reacted by mistake to the weak sound, because a reaction always throws its spark on the drum and thus all mistakes were detected by the experimenter. It is very difficult, as is known, when discriminating, to keep from reacting to the weak as well as the strong sound. By interchanging promiscuously the strong and weak sounds, true discrimination was assured, very few mistakes in reaction to the weak sound occurring. The same apparatus was used in taking simple reaction to sound, using however the loud sound continually.

The Bliss multiple key[2] was used in giving the first time-spark and the stimulus for reaction at the same instant, the circuit producing the sound in the telephone being made at the exact instant the spark circuit was broken.

For Electrical Stimulation.

The apparatus used here was the same as that used in hearing except that two electrodes were substituted for the telephone receiver which served in hearing to give the click. One

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[2] *Studies from the Yale Psych. Lab.* Vol. I, p. 11.

of the electrodes covered the finger-rest of the reaction key and the second was held in the left hand of the subject. Placing the index finger of the right hand on the electrode on the reaction key prepared the subject for both stimulation and reaction. He was asked to react as soon as he felt the shock in his finger. By means of the slide inductorium the strength of the shock could be regulated in the same manner as the two intensities of sound in the apparatus for hearing described above. In discriminating for the two intensities of stimulation the subject was required to react only to the stronger of the two, and not to react when stimulated by the weaker. In simple reaction to electrical stimulation the stronger stimulus was used, as was the case in hearing.

For Touch.

Here Scripture's touch key was substituted in the primary circuit of the spark coil in place of the multiple key used for hearing and electrical stimulation. This key has a hard rubber touch knob fastened on the end of a flexible arm so that a very small amount of pressure applied to the knob raises the arm and breaks the circuit, which causes a spark to pass to the drum. Each time the hand is struck the stimulus to touch is then given at the same time that the circuit is broken giving the first spark on the drum, the second spark being given by the reaction of the subject experimented upon. Here also intensity was used for discrimination. The subject was asked to react when he received the stronger of two intensities of pressure and not to react to the weaker, the intensity being regulated by the strength of blow struck upon the hand. The subject's left hand was used for stimulation and the right for the reaction, the eyes being closed.

For Sight.

In taking the reaction and discrimination for sight a remodeled form of Jastrow's instantaneous exposure apparatus was used and substituted in the primary circuit of the spark coil where the touch key was in the apparatus for touch, described

above. This apparatus is so made that the circuit is broken at the instant a color is exposed, by withdrawing a moveable screen, the moving of which breaks the primary circuit of the spark coil. For determining the length of time required in discrimination, the two colors, blue and red, were used and the subject requested to react whenever blue appeared on removal of the screen and not to react when red appeared, he being unable to tell which color would appear, this being regulated by the experimenter at will. For simple reaction to sight the blue was always made to appear and the subject was asked to react each time it appeared.

RESULTS.

In taking the data in the two series for sound, light, pressure and electrical stimulus, as well as in the practice with sound stimulus, the subject was required to react from fifty to sixty times at each sitting. A rest was allowed between the records taken on reaction with discrimination and choice and those on simple reaction so as to eliminate fatigue. The mean of each set was taken for the final result represented in the tables and charts, each point being the mean of about thirty-five single reactions, which were always those taken at the beginning of the sitting. All data are given in thousandths of a second.

All three subjects reacted most quickly to electrical stimulation and then to sound, pressure, and light respectively. The reaction time was reduced with all three subjects. In order to express the decrease with uniformity, it was reduced to per cent. of decrease over the first reaction. These ratios are shown in Table VII. In general there is a gradual decrease also in the length of time required by all subjects for reaction with discrimination and choice, and likewise for discrimination and choice alone, which was obtained by subtracting the simple reaction time from the reaction with discrimination and choice. Tables I, II, and III give the data for simple reaction time of the respective subjects to the four different kinds of stimuli named above. Tables IV, V, and VI give the same for reaction with discrimination and choice.

EXPLANATION OF TABLES I TO VII.

All times are given in thousandths of a second.

H, reaction-time with sound stimulus.
HV, mean variation for reaction with sound stimulus.
E, reaction-time with electrical stimulus.
EV, mean variation for reaction with electrical stimulus.
T, reaction-time with pressure stimulus.
TV, mean variation for reaction with pressure stimulus.
S, reaction-time with light stimulus.
SV, mean variation for reaction with light stimulus.
h, time of reaction plus discrimination with sound stimulus.
hv, mean variation for reaction plus discrimination with sound stimulus.
e, time of reaction plus discrimination with electrical stimulus.
ev, mean variation for reaction plus discrimination with electrical stimulus.
t, time of reaction plus discrimination with pressure stimulus.
tv, mean variation for reaction plus discrimination with pressure stimulus
s, time of reaction plus discrimination with light stimulus.
sv, mean variation for reaction plus discrimination with light stimulus.

TABLE I.

J. A. G. *Time of Reaction.*

Day	*H*	*HV*	*E*	*EV*	*T*	*TV*	*S*	*SV*
1	143.0	19	129.5	10	182.5	38	200.0	32
2	130.2	16						
3	131.5	13						
4	127.8	17						
5	125.5	13						
6	130.0	13						
7	126.5	16						
8	125.5	8						
9	123.0	8						
10	119.0	8						
11	118.3	13						
12	120.5	11						
13	125.8	8	132.3	12	151.5	16	194.5	18

It will be seen by reference to Table VII, which records the percentage of gain by practice, that in reaction with discrimination and choice, there is a very large gain in the case of J. A. G. and G. C. F. and a very small gain in the case of J. C. P. The latter, it will be remembered, was trained only in simple reaction, while the others were trained also in reaction with discrimination and choice, showing that practice in reaction alone does not greatly lessen discrimination and choice

TABLE II.

G. C. F. *Time of Reaction.*

Day	*H*	*HV*	*E*	*EV*	*T*	*TV*	*S*	*SV*
1	153.0	22	139.0	37	185.0	24	224.8	38
2	153.5	20						
3	140.0	23						
4	136.4	13						
5	132.0	7						
6	127.8	11						
7	136.0	12						
8	118.0	15						
9	122.0	13						
10	122.0	13						
11	126.0	8						
12	119.4	10						
13	117.8	13	110.0	13	166.0	11	123.5	25

For explanation of the table see page 67.

TABLE III.

J. C. P. *Time of Reaction.*

Day	*H*	*HV*	*E*	*EV*	*T*	*TV*	*S*	*SV*
1	136.0	16	135.0	13	141.0	10	155.5	31
2	135.0	13						
3	133.0	11						
4	126.0	10						
5	132.0	10						
6	124.0	6						
7	123.3	8						
8	122.0	7						
9	119.5	7						
10	120.0	13						
11	119.0	7	112.8	4	133.0	14	138.0	14

For explanation of the table see page 67.

time. This is further shown by Table VIII. In reaction time the gain is very marked for all three, but in time of reaction with discrimination and choice, the gain for J. C. P. is very small compared with that of J. A. G. and G. C. F. In fact his time of reaction with discrimination and choice is reduced relatively but very little more than the reduction in reaction alone. As given in Table VIII, in time of discrimination, J. A. G. and G. C. F. make a gain of 137.3 and 85.8 thousandths of a second by practice with sound stimulus, while J. C. P.

TABLE IV.

J. A. G. *Time of Reaction with Discrimination and Choice.*

Day	*h*	*hv*	*e*	*ev*	*t*	*tv*	*s*	*sv*
1	292.0	48	243.0	75	243.5	62	400.0	46
2	311.5	71						
3	261.0	53						
4	265.5	51						
5	187.0	32						
6	157.0	29						
7	178.0	24						
8	137.2	24						
9	139.0	21						
10	135.0	15						
11	136.0	11						
12	138.3	15						
13	137.5	23	158.0	32	222.0	37	343.0	32

For explanation of the table see page 67.

TABLE V.

G. C. F. *Time of Reaction with Discrimination and Choice.*

Day	*h*	*hv*	*e*	*ev*	*t*	*tv*	*s*	*sv*
1	256.0	42	385.0	84	355.0	64	373.0	72
2	234.0	49						
3	195.5	39						
4	200.0	36						
5	177.0	45						
6	165.8	34						
7	169.8	35						
8	158.0	22						
9	137.5	24						
10	138.0	19						
11	147.5	16						
12	143.8	14						
13	135.0	13	152.5	35	220.6	24	245.5	35

For explanation of the table see page 67.

gains only 6.2 thousandths of a second, which is less than half the gain by simple reaction. In the time of reaction with discrimination and choice, while J. A. G. and G. C. F. gain 154.5 and 121.5, J. C. P. gains only 23.5 thousandths of a second and this gain is largely the gain in reaction time, which was 17.0 thousandths. However, with all the subjects the discrimination time was reduced by the practice and markedly so in the first two, who practiced in both reaction and discrimination.

Table VI.

J. C. P. *Time of Reaction with Discrimination and Choice.*

Day	*h*	*hv*	*e*	*ev*	*t*	*tv*	*s*	*sv*
	171.0	55	220.0	45	197.0	41	305.0	43
11	147.8	28	167.8	23	189.0	31	248.0	59

For explanation of the table see page 67.

Table VII.

Per Cent. of Time Gained by Practice.

	Reaction.				*Reaction with Discrimination.*				*Discrimination.*			
	H	*E*	*T*	*S*	*h*	*e*	*t*	*s*	*h'*	*e'*	*t'*	*s'*
J. A. C.	12	0	17	3	53	35	9	14	92	78	0	26
G. C. F.	23	21	10	45	47	60	38	34	83	64	56	18
J. C. P.	13	16	6	11	14	24	4	19	18	35	22	26

J. C. P. was practiced only in reaction time while the other two were practiced in both reaction and reaction with discrimination and choice. All figures of the above table represent per cent. of gain by practice.

Table VIII.

(*a*) *Time of Reaction.*

	J. A. G.				G. C. F.				J. C. P.			
	H	*E*	*T*	*S*	*H*	*E*	*T*	*S*	*H*	*E*	*T*	*S*
B.	143.0	129.5	182.5	200.0	153.0	139.0	185.0	224.8	136.0	135.0	141.0	155.5
A.	125.8	132.3	151.5	194.5	117.8	110.0	166.0	123.5	119.0	112.8	133.0	138.0
G.	17.2	—2.8	31.0	5.5	35.2	29.0	19.0	101.3	.17.0	22.2	8.0	17.5

(*b*) *Time of Reaction with Discrimination and Choice.*

B.	292.0	243.0	243.5	400.0	256.0	385.0	355.0	373.0	171.0	220.0	197.0	305.0
A.	137.5	158.0	222.0	343.0	135.0	152.5	220.6	245.5	147.8	167.8	189.0	248.0
G.	154.5	85.0	21.5	57.0	121.0	232.5	134.4	127.5	23.2	52.2	8.0	57.0

(*c*) *Time of Discrimination.*

B.	149.0	113.5	61.0	200.0	103.0	146.0	170.0	148.2	35.0	85.0	46.0	149.5
A.	11.7	25.7	70.5	148.5	17.2	42.5	74.6	122.0	28.8	55.0	36.0	110.0
G.	137.3	87.8	—9.5	51.5	85.8	103.5	95.4	26.2	6.2	30.0	10.0	39.5

B, thousandths of a second required before practice.
A, thousandths of a second required after practice.
G, thousandths of a second gained by practice.
H, with sound stimulus.
E, with electrical stimulus.
T, with pressure stimulus.
S, with light stimulus.

The practice in reaction with sound stimulus not only reduced the time of reaction with that stimulus but also reduced the time with other stimuli without practice in the latter. The reduction was not the same for all stimuli but it was so large that it was in general nearly equal to the reduction in reaction to the stimulus used in practice. The same thing is true also with regard to reaction with discrimination and choice and also of discrimination alone for J. A. G. and G. C. F. All subjects used sensory reaction in all the experiments.

The data of Tables I to VI are shown in graphic form in Charts I to VI, which are explained on page 76. In general the experiments seem to justify the following conclusions: 1. That practice in reaction to sound reduces the time of reaction to other forms of stimuli by amounts almost equal to the reduction of the time of sound reaction itself. 2. That such practice alone does not reduce the time of discrimination and choice. 3. That practice in discrimination of sounds reduces also the time of discrimination for other forms of stimuli.

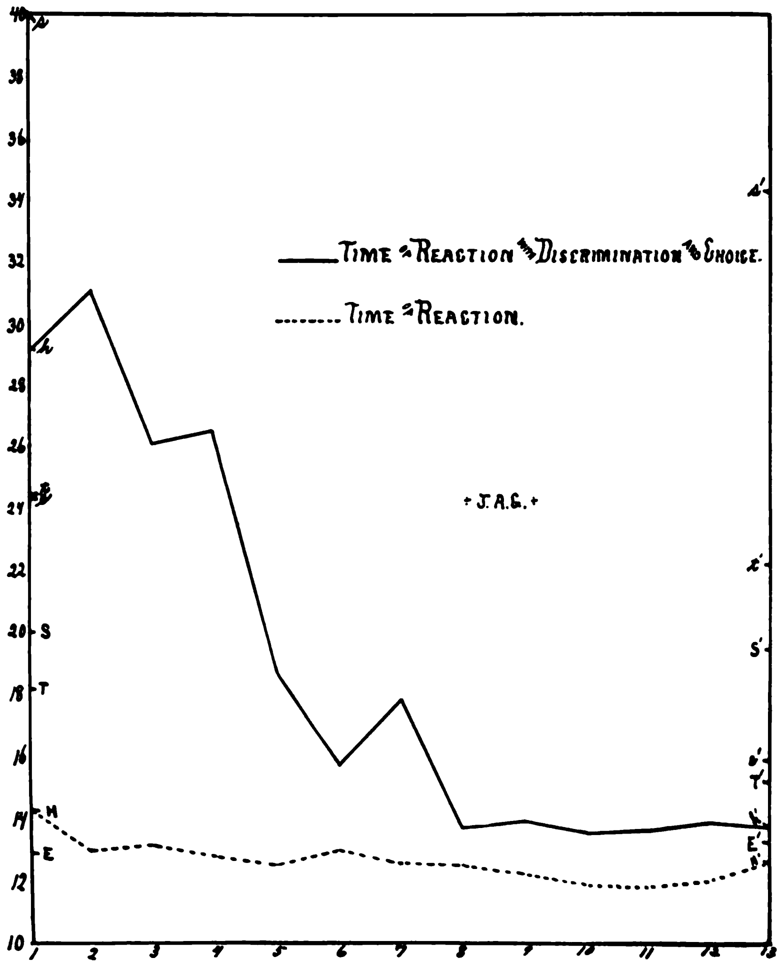

CHART I.

For explanation of charts see page 76.

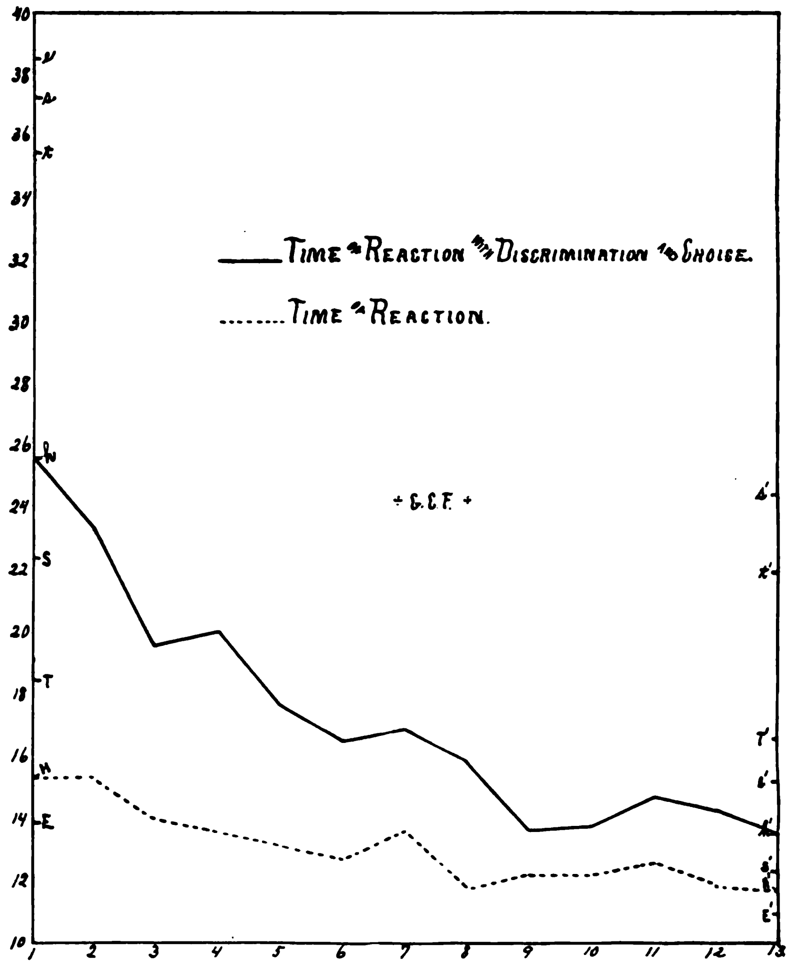
Time of Reaction with Discrimination and Choice.
Time of Reaction.
+ G.C.F. +
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CHART II.

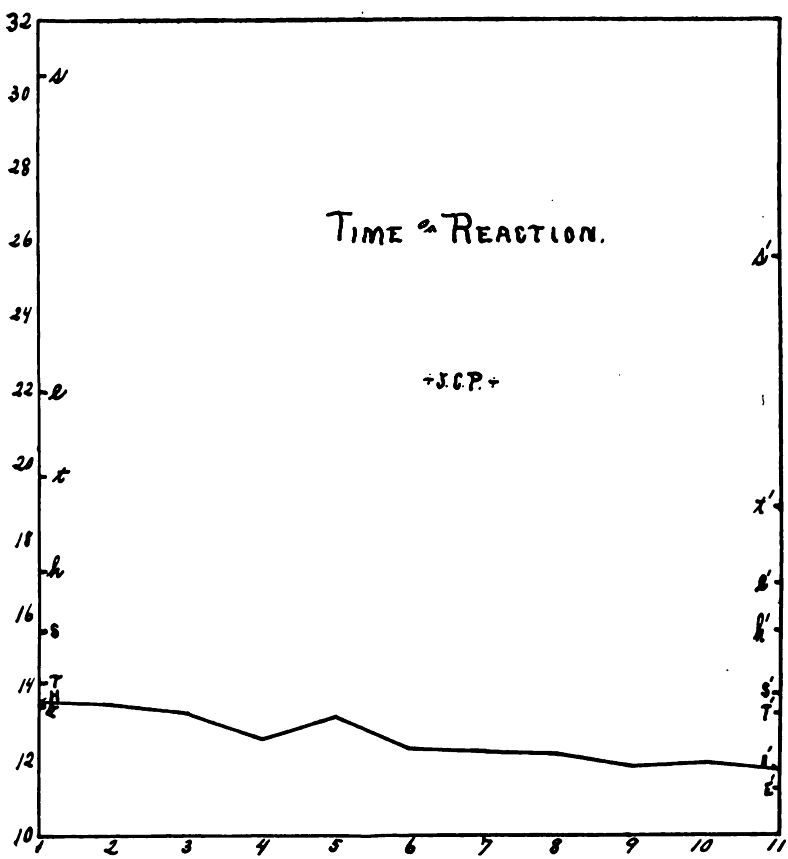

CHART III.

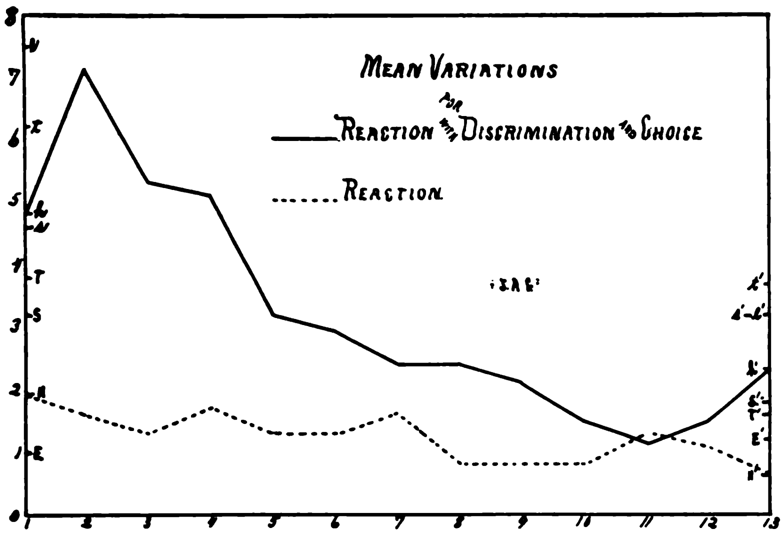

Chart IV.

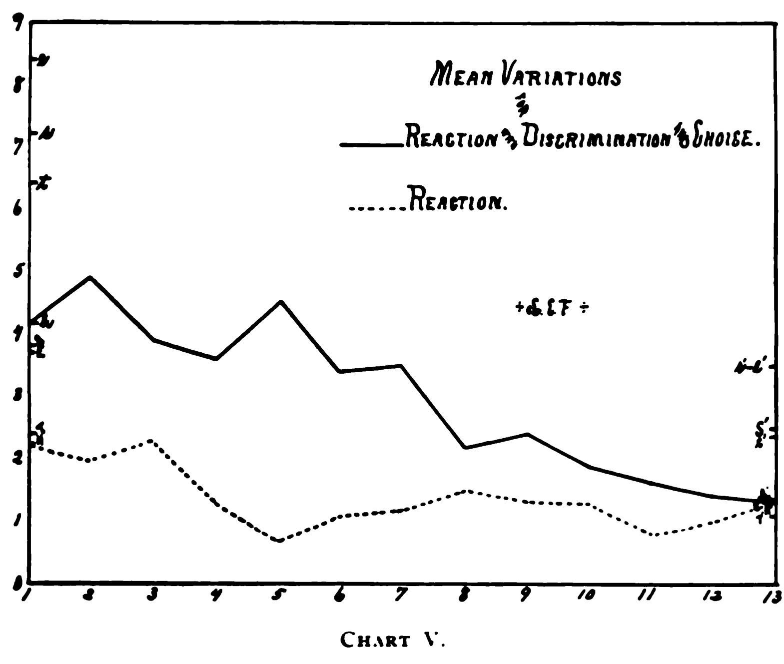

Chart V.

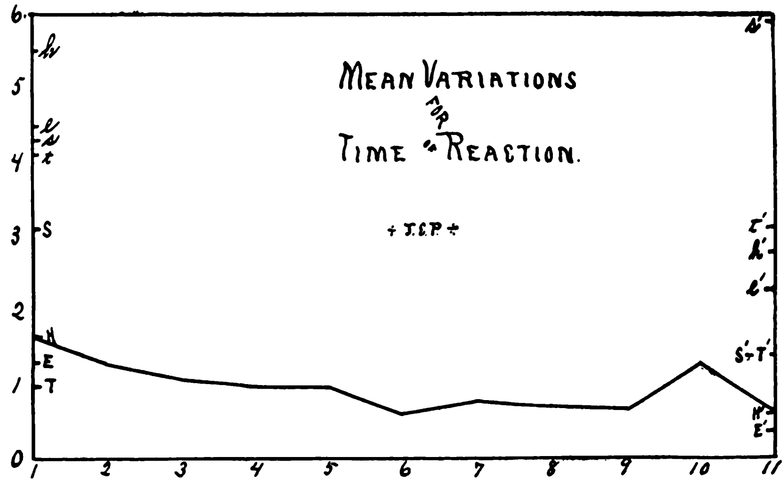

Chart VI.

Explanation of the Charts.

The figures along the line of abscissae indicate the number of days included in the time of practice with sound stimulus. The figures along the line of ordinates indicate time in hundredths of a second.

H, time of reaction to sound before practice.
H', time of reaction to sound after practice.
E, time of reaction to electrical stimulus before practice.
E', time of reaction to electrical stimulus after practice.
T, time of reaction to pressure before practice.
T', time of reaction to pressure after practice.
S, time of reaction to light before practice.
S', time of reaction to light after practice.
h, time of reaction with discrimination and choice with sound before practice.
h', time of reaction with discrimination and choice with sound after practice.
e, time of reaction with discrimination and choice with electrical stimulus before practice.
e', time of reaction with discrimination and choice with electrical stimulus after practice.
t, time of reaction with discrimination and choice with pressure before practice.
t', time of reaction with discrimination and choice with pressure after practice
s, time of reaction with discrimination and choice with light before practice.
s', time of reaction with discrimination and choice with light after practice.

FATIGUE IN SCHOOL CHILDREN.

A REVIEW OF THE EXPERIMENTS OF FRIEDRICH AND EBBINGHAUS.

Untersuchungen über die Einflüsse der Arbeitsdauer und der Arbeitspausen auf die geistige Leistungsfähigkeit der Schulkinder. Von JOHANN FRIEDRICH. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. Bd. XIII. Heft 1 und 2.

Über eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre Anwendung bei Schulkindern. Von H. EBBINGHAUS. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. Bd. XIII. Heft 6.

THERE has recently been much discussion of the question at what hours of the school-day children are at their best mentally. I have even heard it given as a result of "child study" that the last periods of the afternoon are the hours when the children are least easily fatigued and best able to undertake the severer tasks. Educators are even more divided upon the question of the retention or the abolition of the old fashioned recess. A series of experiments lately undertaken by Johann Friedrich, of Würzburg, to show the progress of fatigue in school children during school hours and the influence of recess in decreasing fatigue, are of sufficient interest to merit a short report in these studies. A somewhat similar investigation by Ebbinghaus with special reference to methods of studying fatigue in school children may also be noticed. These experiments may be considered as a continuation of those made by Burgerstein, Griesbach, Miss Holmes, and others. But the methods have been improved and the results are more definite.

Friedrich used two methods. By the first method a series of dictation exercises was given to the children and the number of errors made in writing them was taken as an index of fatigue. These tests extended over a period of six weeks. By the second method the children were required to add and multiply series of numbers, the errors being the index of fatigue. The experiments were made upon 51 school children in the fourth school year, their average age being 10 years. Both the dictation and the number exercises were similar to those of the daily work of the school, so that these tests are free from the objections made against the experiments upon fatigue undertaken by Burgerstein and others. The dictation exercises consisted each of 12 easy sentences, such as, *Der Aal ist ein langer Fisch. Im Frühling freuen sich die Leute.* Each exercise thus contained about 300 letters. At each test, therefore, about 15,300 letters were written by the 51 children together. The papers were collected and the mistakes counted, separate records being kept of the mistakes in each of the 12 sentences in each exercise. Wrong letters, omitted letters, and inserted letters and signs, each counted one mistake. So also did inserted or omitted words. Every word was familiar to each of the children. The instructor pronounced each sentence distinctly. It was then repeated by two children and then at a given signal written down. After writing, the children were required to raise the eyes and were not allowed to look over what they had written. Each sentence required 2½ minutes and the whole exercise, 30 minutes.

The regular school hours of the children were from 8 A. M. to 11 A. M. and from 2 P. M. to 4 P. M.

Test No. I was made at 8 A. M. at the beginning of a day's work.

Test No. II was made (on another day) at 9 A. M. after one hour's work upon mental and practical arithmetic.

Test No. III was made at 10 A. M. after two hours' work upon arithmetic and biblical history, *with an eight minutes' recess* between the two hours.

Test No. IIIa was made at the same hour as No. III, after a similar two hours' work but *without any recess.*

Test No. IV was made at 11 o'clock, after three hours' work upon arithmetic, reading, and religion, *with two fifteen minute recesses* one at the end of each hour.

Test No. IVa was made under conditions like those of No. IV, except that only one *recess of fifteen minutes* was given at the end of the second hour.

Test No. IVb was also like IV, except that *no recess was given*, the children having worked three hours continuously.

Test No. V was made at 2 P. M. at the beginning of the afternoon session.

Test No. VI was made at 3 P. M. after one hour's work at gymnastic exercises.

Test No. VII was made at 4 P. M. after two hours' work in writing and geography, *with a recess of fifteen minutes* between the two hours.

Test No. VIIa was made at 4 P. M., the conditions being similar to those of No. VII except that *the recess was omitted.*

With these tests it is seen that we have a means of noting the progress of fatigue through the morning and afternoon hours, of comparing the morning hours with the afternoon hours, and finally of determining the effects of intermissions for recreation.

The results of these tests are exhibited in Table I, selected from the numerous tables given by the author. I will summarize them below. To verify the results of these tests, the author made another series of experiments, using in place of the dictation exercises, examples in simple addition and multiplication. Each test comprised two examples like the following:

$$\begin{array}{r} 27583140693501894726 \\ +69415258070769412835 \\ \hline \end{array}$$

$$\begin{array}{r} 27583140693501894726 \\ \times 2 \\ \hline \end{array}$$

TABLE I.

Experiment.	Hour.	Recess.	Number of Letters Written.	Errors		Number of Children Making No Errors.	
				Whole Number.	Per cent.	Whole Number.	Per cent.
I.	8 A.M.		15249	33	0.216	37	72.5
II.	9 A.M.		15351	58	0.377	31	60.7
III.	10 A.M.	8 min. Recess at 9.	15351	103	0.671	18	35.2
III. *a.*	10 A.M.	No Recess.	15857	133	0.838	14	27.4
IV.	11 A.M.	Two Recesses of 15 min. at 9 and 10.	15351	96	0.625	18	35.2
IV. *a.*	11 A.M	One Recess of 15 min. at 10.	15351	152	0.990	12	23.5
IV. *b.*	11 A.M	No Recess.	15912	162	1.018	10	19.6
INTERMISSION OF THREE HOURS.							
V.	2 P.M.		15249	35	0.229	33	64.7
VI.	3 P.M.		15351	127	0.827	15	29.4
VII.	4 P.M.	15 min. Recess at 8.	15351	87	0.566	23	45.0
VII. *a.*	4 P.M.	No Recess.	15198	166	1.092	10	19.6

To make all the tests of equal difficulty, the same figures were used in changed order. The examples were given to the children printed in large type upon slips with spaces left for the results. Exactly 20 minutes were allowed for each exercise, containing twenty additions and twenty multiplications. The results of the tests by this second method are exhibited in Table II. They agree in all the essential points with the results shown by the first method.

A summary of the results of the experiments, with the author's conclusions, may be stated as follows: 1. The quality

TABLE II.

Experiment.	Hour.	Recess.	Whole Number of Figures.	Errors.		Number of Children Making No Errors.	
				Whole Number.	Per cent.	Whole Number.	Per cent.
I.	8 A.M.		9112	112	1.229	11	21.5
II.	9 A.M.		10326	179	1.733	10	19.6
III.	10 A.M.	8 minutes Recess at 9.	10258	201	1.959	5	9.8
III. *a*.	10 A.M.	No Recess.	10215	207	2.026	6	11.7
IV.	11 A.M.	Two Recesses of 15 min. at 9 and 10.	10378	201	1.936	7	13.7
IV. *a*.	11 A.M	One Recess of 15 min. at 10.	10326	230	2.228	1	1.9
IV. *b*.	11 A.M.	No Recess.	10366	236	2.276	4	7.8
INTERMISSION OF THREE HOURS.							
V.	2 P.M		10380	186	1.791	10	19.6
VI.	3 P.M.		9669	199	2.058	1	1.9
VII.	4 P.M.	15 min. Recess at 3.	10357	218	2.104	6	11.7
VII. a.	4 P.M.	No Recess.	10428	251	2.406	4	7.8

of the work done by school children decreases with the increase of the school hours. The best work is done at the beginning of the school session and the poorest at its close. 2. If we compare the work of the morning session with that of the afternoon, the quality of the former is in every instance better. Even a three hours' intermission at noon is not sufficient to reinstate the freshness of the morning. At the close of a two hours' session in the afternoon the children are in a worse condition than at the close of a three hours' morning session. 3. The influence of a recess is in every instance to increase

the quality of the work, and in a three hours' session, two recesses increase it more than one. For instance, one recess at 10 decreases the errors at 11 from 162 to 152, and two recesses, at 10 and 9, decrease them to 96. In addition to these results, the author compared the quality of the work done in the first half of each experiment with that of the second half, with the result that the best work was uniformly done in the former. It will be remembered that the dictation exercises required 30 minutes and the number exercises 20 minutes. Finally, although the author himself does not call especial attention to this fact, it may be seen from Table I, that the quality of mental work was lowered and not raised by an hour's instruction in gymnastics from 2 to 3 P. M. and indeed lowered very remarkably. This appears to have been work for the children and not rest or play.

The author's practical conclusions are as follows: School instruction is for the mentally and physically growing child *work*, and consumes his mental energy. If it become *overwork*, it checks his mental and physical development. It is shown by these and other experiments and insisted on now by many educators, that short intensive study hours are better than long hours. Especially with children in the lower grades, fatigue increases very rapidly with the continuation of instruction. The child should be granted a recess of from eight to fifteen minutes after every sixty minutes, the time to be spent in attention to bodily needs, to rest, and to the taking of nourishment. The severer studies should find a place in the earlier morning hours. Whether there should be any afternoon session at all is questionable. At any rate, only light exercises, such as penmanship, singing, etc., should be permitted in the afternoon.

The study made by Ebbinghaus was the outgrowth of the appointment of a commission to investigate the subject of fatigue in school children, the occasion being certain alarming symptoms of fatigue and nervous exhaustion in the schools of the city of Breslau, in connection with the five hour morning session. Owing to certain objections to all the methods which

have been used in studying fatigue and mental ability of school children, Dr. Ebbinghaus devised a new method called the "combination method." An easy passage of prose was mutilated by the omission of certain words, syllables, and letters, the omitted portions being indicated by dashes. The passages thus prepared were given to the children upon printed slips, and they were then required to fill out the omissions as quickly as possible so as to restore the sense of the passage and to furnish the requisite number of words, syllables and letters. Selections from a German translation of Gulliver's Travels were thus prepared for the younger classes, and selections somewhat more difficult for the older classes, care being taken to provide passages of like difficulty to be used at the different hours with the same children. Five minutes were given for the test. Both the amount of work accomplished by the child during the five minutes and the number of errors made were taken as an index of his mental ability and condition at the time. This test was not designed for children under ten years of age.

The commission decided to use conjointly this method and two others. The other methods were the "computation method" used by Burgerstein, and the "memory method" used by Bolton at Worcester, Massachusetts. (See *Amer. Journ. Psych.*, Vol. IV, p. 362.) According to Burgerstein's method, the children were given easy exercises in addition and multiplication. This test as used by Ebbinghaus occupied ten minutes and was made, as in the case of the other methods, at the beginning of the school session and at the end of each hour thoughout the morning. As in the other method, both the work accomplished and the errors made were taken into account. By the third, the "memory method," a series of words were dictated to the children and they were required immediately to write them down from memory. The words used were the numbers from naught to twelve inclusive, (all pronounced as one word in German) arranged in unusual orders, and dictated to the children in series of 6, 7, 8, 9, and 10. In such a memory-span, the younger children can at any

rate grasp 6, while 9 and 10 cannot be grasped without errors even by the best and oldest children. Ten such series, two each of 6, 7, 8, 9, and 10 words, were given to the children at the several hours named, the errors serving as a measure of mental ability and fatigue.

These three methods were tried one at a time upon three Wednesdays, each two weeks apart, upon twenty-six classes of girls and boys, ranging from ten to eighteen years of age, in a *Gymnasium* and a higher girls' school at Breslau. Between 11,000 and 12,000 papers were thus secured. The data were susceptible of several uses. First the mental ability of the upper and lower classes could be compared. Second, by dividing each school into three divisions, according to the standing of the children, it was possible to compare memory-span, fatigue, etc., of the bright, average and dull children. Third, comparison could be made of the mental ability of boys and girls. Fourth, it was possible to determine the progress of fatigue with the hours of school instruction. The results may be summarized as follows: As might be supposed, the mental ability of the children increased from class to class in respect to memory, computations and the restoration of the omitted words and syllables The rule was not without exceptions in particular instances. The advance with age was most marked in the combination method, the restoration of the passage requiring more power of thought. In regard to the second object, the comparison of the bright, average, and dull children, some unexpected results were obtained. In the memory test there was no marked difference between the three classes of children, the difference, if any, being in favor of the dull ones. In the mathematical tests, the bright children computed more figures and made fewer errors than either the average or the dull, but the difference between the average and the dull children was less marked. The third method, the combination method, revealed the greatest difference between the bright, average, and dull children. The first third made fewer errors, while accomplishing more work than the second, and the second showed a like superiority over the third.

Comparing the mental ability of the boys and girls, in the lower classes, the boys of a given age displayed superiority over the girls of like age uniformly and in a marked degree in all three methods. For instance in the memory method, boys of 10 years made 17.8 errors, while the girls of 10 years made 29.6 errors. The boys of 11 years made 17.5 errors, while the girls made 21.5 errors. In the upper classes comparison of the boys and girls is more difficult, as the school system is different for the sexes. Comparing the highest class in the girl's school with the *Untersecunda 1* of the *Gymnasium* the class most nearly corresponding, the girls are somewhat behind, although in one method, the computation method, they excelled the boys in the amount of work accomplished. We observe, however, that the average age of the girls of this class is 15.6, while that of the boys is 17.1. If now we compare ages instead of classes, the girls show decided superiority.

Finally we may observe the results of Ebbinghaus' experiments upon fatigue. The tests were made upon the children at the beginning of a five hours' morning session and at the end of each hour, thus making six tests on the same day. The objection to this method of studying fatigue, that the effects of practice will at least partly compensate for the effects of fatigue, is discussed by the author, but its full importance does not seem to be realized. As the tests were made upon the same day, with the progress of the experiments a considerable increase in expertness as a result of practice would tend to increase the amount of work done and decrease the number of errors. By the memory method, most of all, one would expect practice to improve the quality of the results. We are not surprised, therefore, to find the errors actually decreasing with the progress of the morning instead of increasing as a result of fatigue. To make the matter worse, it is admitted by the author that the children bent themselves with great energy to the tasks because the report had been circulated, at least in some of the classes, that the reinstatement of the afternoon session would depend upon the results of these tests. It is difficult to see how experiments carried on under these condi-

tions could be of any value in determining the progress of fatigue.

The results were, however, in brief as follows: By the memory method, the errors at the end of the fourth and fifth hours were decidedly less than at the beginning of the first and second hours. By the computation method, while the amount of work accomplished was greater at the end of the fifth hour, the per cent. of errors increased from 1.1 to 1.9. By the third method, upon which more emphasis is placed by the author, as being a better test of mental ability and freshness, the results of fatigue are more marked. Notwithstanding the influence of practice, the errors increase markedly with a few exceptions from hour to hour, and especially in the lower grades. On the other hand, the amount of work accomplished increases somewhat and irregularly *except* in the lower grades. While all classes show the effect of fatigue, the effects are most marked upon the lower classes. The author calls attention to the obvious fact that the mere presence of fatigue in school children in a five hours' school session is nothing either abnormal or alarming. He admits, however, that the several imperfections in his methods have not permitted him to ascertain the real amount of that fatigue. To the present writer it seems that, while as a study of methods and as a means of ascertaining mental differences between school classes and between the sexes, this research is important as well as interesting, as a study of fatigue it is of little or no value.

G. T. W. P.

RIGHT-HANDEDNESS AND LEFT-HANDEDNESS.

A REVIEW OF RECENT WRITINGS.

Rechtshändigkeit und Linkshändigkeit. Von DR. M. ALSBERG. Hamburg. 1894. Pp. 32.

Die linke Hand. Von DR. L. W. LIERSCH. Berlin, 1893. Pp. 47.

The Origin of Right-Handedness. Mental Development in the Child and the Race. By JAMES MARK BALDWIN. Chapter IV. Pp. 58-80. Macmillan & Co.

Left-Handedness. By F. TRACY. Transactions of The Illinois Society for Child-Study. Vol. II, No. 2. Edited by C. C. Van Liew. Pp. 68–76.

The Right Hand: Left-Handedness. By SIR DANIEL WILSON. Macmillan & Co., 1891. Pp. 215.

SOME of the more important questions which the above writings attempt to answer are the following: What is the explanation of the exception to bilateral symmetry which we have in right-handedness? Granting an answer to this question, what are the causes which have led to the selection of the right hand rather than the left? Is right-handedness inherited, or is it a habit acquired by the individual child? Is right-handedness an advantage, or should ambidexterity be encouraged as much as possible? What shall the mother do with the left-handed child in the first years of the child's life? What shall the teacher of writing do with the left-handed pupil?

A brief summary of Dr. Alsberg's work is as follows: Right-handedness is commonly thought to be due to custom

and habit. This is incorrect. Right-handedness is found to be universal, common to all people and all ages. The left pointing profiles of figures cut in bone by the primitive man of the reindeer age show him to have been right-handed. The earliest languages are corroborative of the same opinion. Left-handed people have always constituted a small minority, according to Hyrtl, 2%, according to Ogle, 4½% according to Liersch, 3% to 4%.

That right-handedness is not due to custom or habit is shown not only by its universality but also by the fact that education and practice have so little power to change either right- or left-handedness. In spite of all training, and in spite of all the disadvantages incident to the use of tools, pen, etc., a naturally left-handed person never becomes fully right-handed. Various other theories that have been advanced to explain right-handedness, such as the sword and shield theory,—the primitive man, supposing that his heart is on the left side of the body because it is more plainly felt beating there, protects it by holding his shield in his left hand and so becomes right-handed by the use of his sword in his right hand,—are rejected by the author, and the true explanation of right-handedness he finds in physiological peculiarities. The immediate cause lies in the brain. The left hemisphere of the brain, controlling the right hand, is larger, heavier, and better developed than the right. Here are found the important centers for speech and writing, and it is reasonable to suppose that the left hemisphere being thus specially developed, should assume control of other forms of manual activity. After this apparent begging of the question in his explanation, Alsberg goes on to show why it is that we are left brained. The ultimate cause is found in anatomical conditions. The predominance of the left brain is due to its better nourishment, and this follows from the fact that, owing to a difference in the structure of the right and left common carotid arteries, the left brain receives a larger supply of arterial blood, the left common carotid artery springing *directly* from the arch of the aorta, while the right common carotid springs indirectly therefrom,

the blood first passing into the innominate artery, which branches into the right carotid and right subclavian arteries. The left hemisphere thus receives the blood more directly and under greater pressure. Finally the researches of Hyrtl are quoted to show that anatomical abnormalities in the structure and branching of the arch of the aorta, actually occur in probably sufficient numbers to account for the 2% or 3% of left-handed people.

That which is most noteworthy in the somewhat longer monograph of Dr. Liersch is his earnest plea for the left hand. In all ages the left hand has been held in slight honor. In many languages it is the synonym for awkwardness, clumsiness, and meanness. A reinstatement of the left hand to a position of honor and equality is much to be desired. Our extreme right-handedness has many serious attendant evils. Lateral curvature of the spine, which is usually right sided, is one of them. It is more frequent in girls than in boys, especially between the ages of 7 and 17. It is to a large extent the result of bad sitting postures, due to the predominant use of the right hand during the plastic growing period of school life. The exclusive use of the right hand in writing, together with wrongly constructed seats and desks, has a tendency to produce this deformity especially in the case of weak and anaemic children. The author is particularly bitter against the use of the old slanting script, which in the ways mentioned is responsible for many of these evils, and which fashion still demands because it is thought to be rapid and beautiful. Since, however, writing in any case must be done with one hand, on account of its many attendant evils it is dangerous for young children. This danger could be greatly mitigated by the introduction of vertical writing, by proper desks, seats, and writing postures. The type-writer, since it engages both hands and does not encourage one sided positions, is to be highly commended for children's use.

Right-handedness reacts also injuriously upon the eyes. The right eye is more often found to be weak than the left.

Short-sightedness which is so very common, is largely due to the fact that we compel the children during the early plastic period of life to busy themselves with small objects and letters, especially in writing. The one-sidedness which is encouraged by right-handedness is also a prolific cause of other troubles such as chlorosis, diseases of the heart, intestines, etc. In conclusion the author makes an earnest plea for ambidexterity, and finds nothing but evil to result from the asymmetry of right-handedness. The book is extreme but suggestive nevertheless.

Of the writings under present consideration, Professor Baldwin's chapter on the origin of right-handedness is perhaps the most satisfactory. It is divided into an experimental and a theoretical part. In the first are described some experiments to determine the conditions of the appearance of right-handedness in the author's little girl. These experiments, familiar perhaps to the reader already, need not be described in detail here. They showed apparently that right-handedness developed in the seventh and eighth month of the child's life, and developed as it were spontaneously, independently of experience in the use of either hand. This right-handedness could not have been due to the incidental causes which are often mentioned as the cause of right-handedness, since it developed before these causes, such, for instance, as habits of walking, habits of creeping, or habits of lying in sleep.

In the second part the authór discusses the theory of right-handedness. Dextrality being, as we have seen, something more than an acquired individual habit, we are obliged to seek for its cause in inherited physiological grounds. Since it is probably not present in the lower animals, and since the center for speech and the center for the control of the right hand are closely adjacent in the left hemisphere of the brain, it is probable that speech and right-handedness have arisen together in the development of the human race. In left-handed persons, as is known, the speech center is in the right hemisphere of the brain. It is very probable that the earliest forms of expression were not by means of spoken language but by ges-

ture language, by expressive movements of the hand. Right-handedness was thus merely a form of expressive differentiation, and preceded the more complete differentiation of speech in the left hemisphere. The original cause of this differentiation which has resulted in right-handedness and speech is to be found in spontaneous variation. That such asymmetrical variation should not have taken place among the lower animals is apparent, as it would be disadvantageous to them in movement. With man, however, such variation would be distinctly advantageous as soon as he should begin to use his hands for other purposes than walking, as for instance in fighting.

The view as to the origin of right-handedness which is here so concisely formulated by Professor Baldwin is pretty sure to become the generally accepted one. One wonders indeed at the rather trifling character of much of the speculation about this problem. Professor Baldwin does not discuss the question why the left rather than the right hemisphere came to be selected for the finer differentiated movements, a question wholly unimportant in itself, but answerable perhaps by the anatomical differences mentioned by Alsberg.

Tracy's interest in the subject is pedagogical. A circular was issued to the teachers of Ontario, asking five questions. Is the left-handed child less expert than the right-handed? Is he less adroit in the use of speech? In excitement does he gesticulate with the right or the left hand? Is awkwardness in gesture accompanied by hesitancy of speech? How do teachers try to break left-handed children of the habit and what is the value of the attempt? Only about eighty answers were received to the questions but they expressed a great deal of unanimity. Left-handed children are just as expert as any others, except possibly in writing. They are not less adroit in the use of language. They gesticulate with the left hand in excitement. Awkwardness in gesture and speech are often associated. Attempts to make left-handed children right-handed are usually failures.

It will be seen from these answers that as far as the theoretical question is concerned, they confirm Baldwin's conclusions.

As for the practical questions, although there are, no doubt, among parents and teachers who unthinkingly follow old customs, many who attempt to correct left-handedness, nevertheless as Tracy's study shows, is not only difficult but wholly unnecessary. If it were to be done, it certainly should not be attempted by the teacher after five or six years' use of the left hand has established the structure in the nerve centers. Whether the young child might not, as Liersch hopes, be taught to be ambidextrous is another question. As regards writing, the matter is not quite so easily disposed of. If the left-handed child is to write with the left hand, he ought for his own convenience to be allowed to write from within outward, as the right-handed child does. This would produce *Spiegelschrift*, reversed and reading from right to left. As this is illegible the left handed child must choose between two evils, either writing with the right hand, or if with the left hand, writing from without inward. Although the latter is awkward, it is undoubtedly the less of the two evils.

Sir Daniel Wilson's *The Right Hand: Left-handedness*, is a scholarly work, a rather complete summary of all that was known about the origin, extent, and cause of right-handedness at the time the book was written, in 1891. The chief points of interest in it have been covered in the above notices of the more recent works. The author anticipates Liersch in his argument for ambidexterity.

G. T. W. P.

THE

University of Iowa

Studies in Psychology

EDITED BY

GEORGE T. W. PATRICK,

PROFESSOR OF PHILOSOPHY.

VOLUME II.

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I. VISUAL PERCEPTION OF INTERRUPTED LINEAR DISTANCES.
II. THE MATERIAL-WEIGHT ILLUSION.
III. LOCALIZATION OF SOUND IN THE MEDIAN PLANE.
IV. HEARING-ABILITY AND DISCRIMINATIVE SENSIBILITY FOR PITCH.
V. MOTOR ABILITY, REACTION-TIME, RHYTHM, AND TIME SENSE.

BY
C. E. SEASHORE,
ASSISTANT PROFESSOR OF PHILOSOPHY.

Introduction: The Method.

The problems for the present investigation have been selected with reference to the need of data, their interrelations, and the adaptation of methods and apparatus. Some are involved in standard experiments, some have been developed by other investigators and are here developed a step further, and some are new. Likewise the apparatus consists of some standard pieces, some adaptations and improvements, and some new pieces. Five independent studies are reported together in this article. There is no other reason for grouping them under a common head than that they were made upon the same class of persons and are in the nature of statistics.

The experiments have been made upon the members of the introductory classes in psychology in the University during the academic years '97–8 and '98–9. The students were Juniors and Seniors, and both sexes were represented. These students were fairly prepared for observing and yet did not know what results to expect in any particular test, and where

necessary they could be kept ignorant of the objective relations. All the members of the classes volunteered to make individual appointments for the tests, and the test was omitted upon no person except for such reasons as would be considered valid excuse for absence from the class. In every case we aimed to obtain the most favorable conditions for observation. Therefore the data to be presented are obtained from a well defined class of persons under similar conditions. They represent the best efforts of the university students.

As a rule the problems were not discussed in class prior to the testing. Careful precautions were taken to obviate any suggestion of what might be expected in the results. As the courses were elective, the students were naturally interested in the study of mental phenomena. After the test, copies of the records were furnished to the class, the results were discussed, and each individual could compare his or her record with the records of others. This added materially to the interest in the experiments, which is so essential to painstaking observation. The records often express striking peculiarities of sensibility and judgment and, as it is important that no one should be in danger of having his feelings injured by exposure before class-mates, no names have been given with the records. The students passed by assigned numbers which have been retained in this report as in the original report to the class, so that each student can identify his own but no other record. By means of these numbers, one individual's record may be traced through records of the various tests for which individual records are given. The determination of variations of mental processes with sex constitutes an important feature of this investigation, in which the opportunities and conditions for comparison have been specially favorable.

None of the following problems are treated exhaustively. All the time and energy might well have been devoted to one of the problems or a part of one. But most of the problems, e. g., those on illusions, are of such a nature that the best results can be obtained upon the first trial; then, the students would

have tired of repeated tests of the same kind and the experience would not have been so valuable to them as beginners; and, furthermore, most of the development of a problem can be carried on to the greatest advantage by the ordinary method with trained observers.[1] It is mainly when we wish to determine the nature of the naive experience or the uniformity of a particular tendency that we find it profitable to appeal to the statistical method.

These statistics that have been obtained in the laboratory have been supplemented by some tests made upon school-children. As the account of the latter occurs in fragments under the appropriate topics, it is necessary to state, in this introduction, how the data were obtained.

Six students in a university course in genetic psychology, under the direction of the writer, made these tests upon children in the public schools of Iowa City during the spring term of 1898. These students had previously pursued courses in psychology and pedagogy and undertook the investigation mainly for the sake of the training.

Ten boys and ten girls of each age from six to fifteen, inclusive, were selected from a grammar school and a primary school. Mr. G. C. Fracker, the principal of the grammar school, selected and graded the children. A fair distribution of the children was obtained by selecting, from the registers, those whose birthdays fell nearest the day of the appointment. The respective teachers invited them to come to the school rooms at different periods on Saturday mornings. They were received by a teacher who registered and guided them.

The registers called for the child's name, assigned number, age, sex, grade, standing according to the last monthly report, mental ability as estimated by the teacher, and remarks about known peculiarities. The last three items were obtained at the teacher's leisure. Each child carried a pass-card contain-

[1] The normal person experimented upon in a psychological laboratory is an "observer." He observes under the conditions imposed upon him by the experimenter. Psychologists are in danger of adopting the inappropriate term "subject" from the language of the pathological clinic.

ing the name and the assigned number. The results were classified directly according to problem, individual, sex and age. The experimenters occupied different rooms and the children passed singly from room to room as they were called. The children were called one period in advance of their respective turns so that each one had an opportunity to see another perform the test. Six minutes were allotted for each test; thus each child was required to work thirty-six minutes in all.

The six students pursued different investigations. Five of the six problems which constituted the series are reported in this article, namely: (1) Hearing-ability, (2) Discriminative sensibility for pitch, (3) Illusions of space, (4) Illusions of weight, (5) Illusions of time. In the preliminary work they studied their problems, tested their apparatus, and practiced until they became expert in their respective measurements. The writer is responsible for the method, the apparatus, and the final statement of the results.

All the sections of this report are condensed into a compact form in order that the results may be accessible. Historical discussions are avoided as far as possible. Likewise all discussion of theories is excluded, except in so far as it is necessary for the statement of the problems or follows directly upon the conclusions drawn from the experiments. It is earnestly hoped that this procedure will not be construed as indicating a lack of recognition of what the writer owes to previous investigators.

The writer herewith extends his sincere thanks to all the students who have contributed to the success of these experiments by acting as observers. The names of those students in the laboratory who have assisted by performing certain series of experiments are appended to the corresponding sections of this report, in grateful acknowledgement of the services rendered. The writer most cordially acknowledges his obligations to Professor Patrick for constant promotion of the work.

I. Visual Perception of Interrupted Linear Distances.

In this investigation an attempt is made to determine the extent and the nature of some of the variations of the Müller-Lyer illusion for untrained observers to whom the illusion has not been explained. Other illusions are dealt with incidentally. The tests were made upon the students in the introductory class in psychology before they had studied the subject of illusions. It was, however, apparent to all that some illusion was involved in the test.

First Series: List and Explanation of the Forms.

(Miss Mabel Williams assisted in making this series of tests.)

The numbers in this list refer to the corresponding numbers in the reproductions of the forms on page 9-13. The positions of coins are represented by circles. Where coins were not used the figures are copies of the original figures. The forms in the plates are reduced to one-third of the original size. They are drawn so as to express the results of this test; i. e., they are drawn according to the averages of the estimates recorded in Table I. The short vertical marks indicate the true distances. The distances between these marks and the limiting edges of the end-figures represent the amount of the illusions.

The description of the following forms of figures involving the illusion may be facilitated by observing two fundamental types.

Type A. This is the type of Form 6. Two silver dollars are placed in a horizontal line and separated by a distance equal to the diameter of a dollar. A third dollar is placed so far to the right of these, in the same line, that the distance between the left edge of this and the right edge of the middle dollar shall appear to be equal to the distance between the right edge of the middle and the left edge of the first dollar. That is, the distance between the adjacent edges of the second

and the third shall appear to be equal to the distance between the remote edges of the first and the second. This is the double form of the Müller-Lyer figure. In the standard member the angle-lines point inward and in the other member they point outward. Form 6 shows graphically the average amount of the illusion as determined by the present experiments. The mark to the right indicates where the inner edge of the third dollar should be in order to make the compared distance equal to the standard distance.

Type B. This is the type of Form 22. Two silver dollars are placed in a horizontal line and separated by a distance equal to twice the diameter of a dollar. It is required to indicate the middle point between the extreme left-hand edges of the two coins. The total distance here to be divided is equal to the standard distance in Type A. The division mark is placed according to the present estimates. The actual middle point is not indicated.

Form 1. Five dimes[1] are used. This form varies from Type A in that the standard member is composed of four coins instead of two. The four dimes are placed in a line and separated by inter-spaces equal to the diameter of a dime. The fifth dime is to be placed so that the distance between the adjacent edges of this and the fourth shall appear to be equal to the distance between the remote edges of the first and the fourth. Standard, 124 mm.

Form 2. Four dimes are used. This form differs from Form 1 in that the standard member is composed of three coins instead of four. Standard, 89 mm.

Form 3. Three dimes are used in the form of Type A. Standard, 54 mm.

Form 4. Type A. Silver quarters. Standard, 72 mm.

Form 5. Type A. Silver half-dollars. Standard, 91 mm.

Form 6. Type A. Silver dollars. Standard, 114 mm.

[1] The actual diameters of the coins are as follows: dollar 38.10 mm.; half dollar, 30.48 mm.; quarter, 24.13 mm.; and, dime, 17.78 mm. Sufficient allowances were made in the interspaces of the standard members to eliminate the fractions. The coins were new.

Form 7. This form differs from Type A in that the two members are separated by the duplication of the middle coin. The standard member is composed of two silver dollars as in Form 6. The compared member is formed by placing two dollars in the same line, to the right, at such a distance apart that the distance between these two shall appear to be equal to the distance across the other two.

Form 8. The standard member is retained as in Form 6. A blank paper is placed immediately to the right. It is required to indicate the apparent distance between the two coins by two dots on the blank paper. Standard, 38 mm.

Form 9. This form differs from Form 6 in that the two dollars of the standard member are placed edge to edge. Standard, 76 mm.

Form 10. Type A. This is composed of plain circles, each having an outside diameter equal to the diameter of a dollar. Standard, 114 mm.

Form 11. Type A. The circles are filled with concentric circles which constitute an attractive design. Standard, 114 mm.

Form 12. Type A. This form consists of Form 10 with the addition of a base-line.

Form 13. Type A. The end-figures consist of plain squares. Standard, 114 mm.

Form 14. Type A. The limiting figures consist of isosceles triangles, placed as represented in the cut. The horizontal distance through each triangle is 38 mm.

Form 15. This form differs from Form 14 in the omission of the vertical sides of the triangles. Standard, 114 mm.

Form 16. This differs from Form 10 in that semicircles are used instead of circles. Standard, 114 mm.

Form 17. This consists of Form 16 with the addition of horizontal lines, 19 mm. long, to each end of the semicircles. Standard, 114 mm.

Form 18. This and the next two forms differ from Type A in that the two members are placed in different directions. The standard member consists of two silver dollars as in Form

6. It is required to place a third dollar at such a point above these two that the distance between the adjacent edges of this and each of the other two shall appear to be equal to the horizontal distance in the standard member. Standard, 114 mm.

Form 19. This form differs from Forms 6 and 18 in that the second member is formed by placing the third dollar vertically over the second. Standard, 114 mm.

Form 20. This form differs from Form 6 in that the standard member is placed in a vertical position. The second member is produced by placing the third dollar to the right of the lower dollar in the standard member. Standard, 114 mm.

Form 21. A silver dollar is placed on a blank paper and the observer is required to indicate its diameter by two dots on the paper, to the right of the coin.

Form 22. Type B. Silver dollars. Standard, 57 mm. In this and the following four forms the measurement is made upon the section to the right of the middle point.

Form 23. Type B. Squares, with sides 38 mm. long, constitute the limiting figures. Standard, 57 mm.

Form 24. Type B. The limiting figures consist of angle-lines of the same length and forming the same angle as in Form 15. Standard, 57 mm.

Form 25. Type B. The diameter of the large circle is 114 mm; that of the small circle, 19 mm. They are 114 mm. apart. Standard, 114 mm.

Form 26. Type B. The diameter of the large circle is 152 mm. The inside circle, whose diameter is 19 mm., is 114 mm. to the right of the left hand edge of the large circle. Standard, 57 mm.

Apparatus and Method.

The twenty-six forms were put on separate cards, of a silvery tint, 57 cm. long and 38 cm. wide. Where the coins were not used the moveable end-figure was drawn on a carefully trimmed card of the same color as the background. The adjustment was made by moving the adjustable coin or

PLATE I.

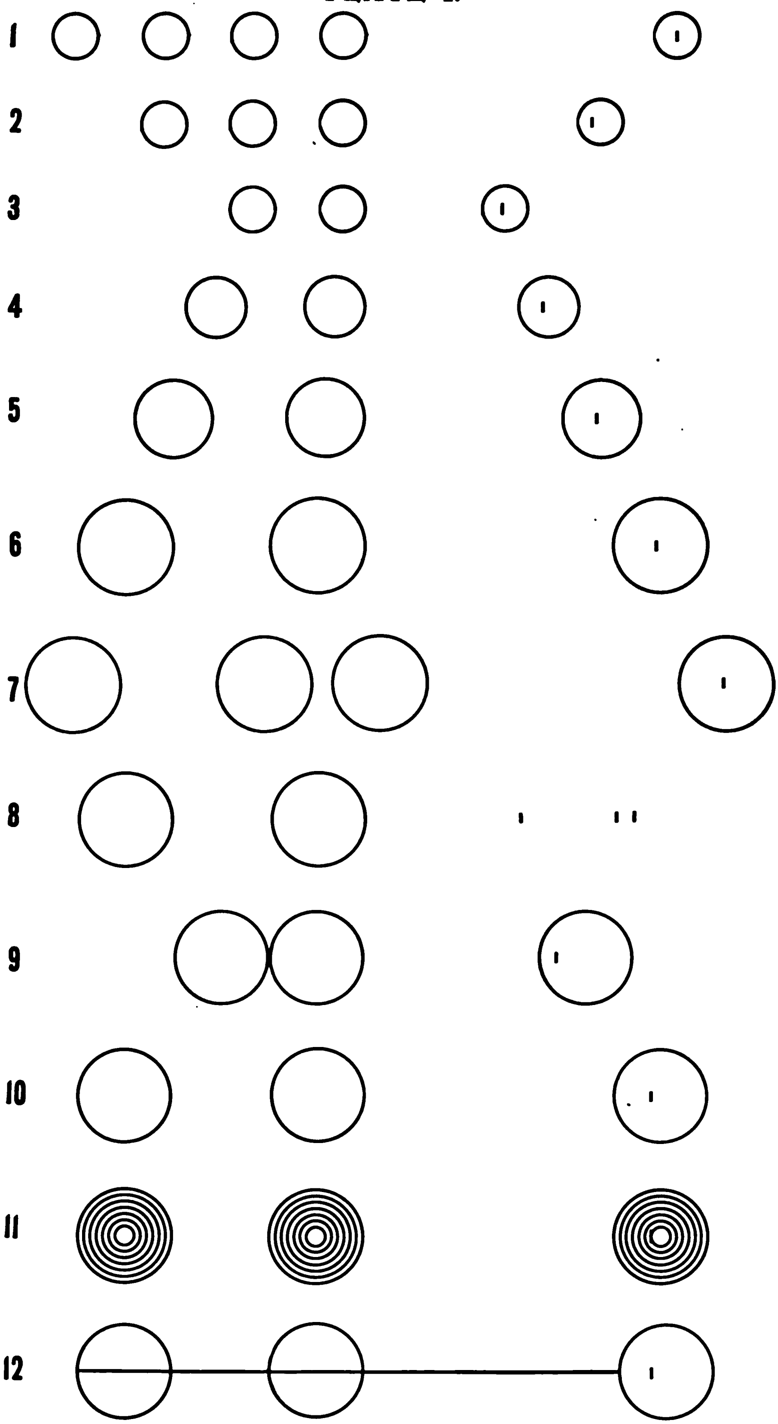


PLATE II.

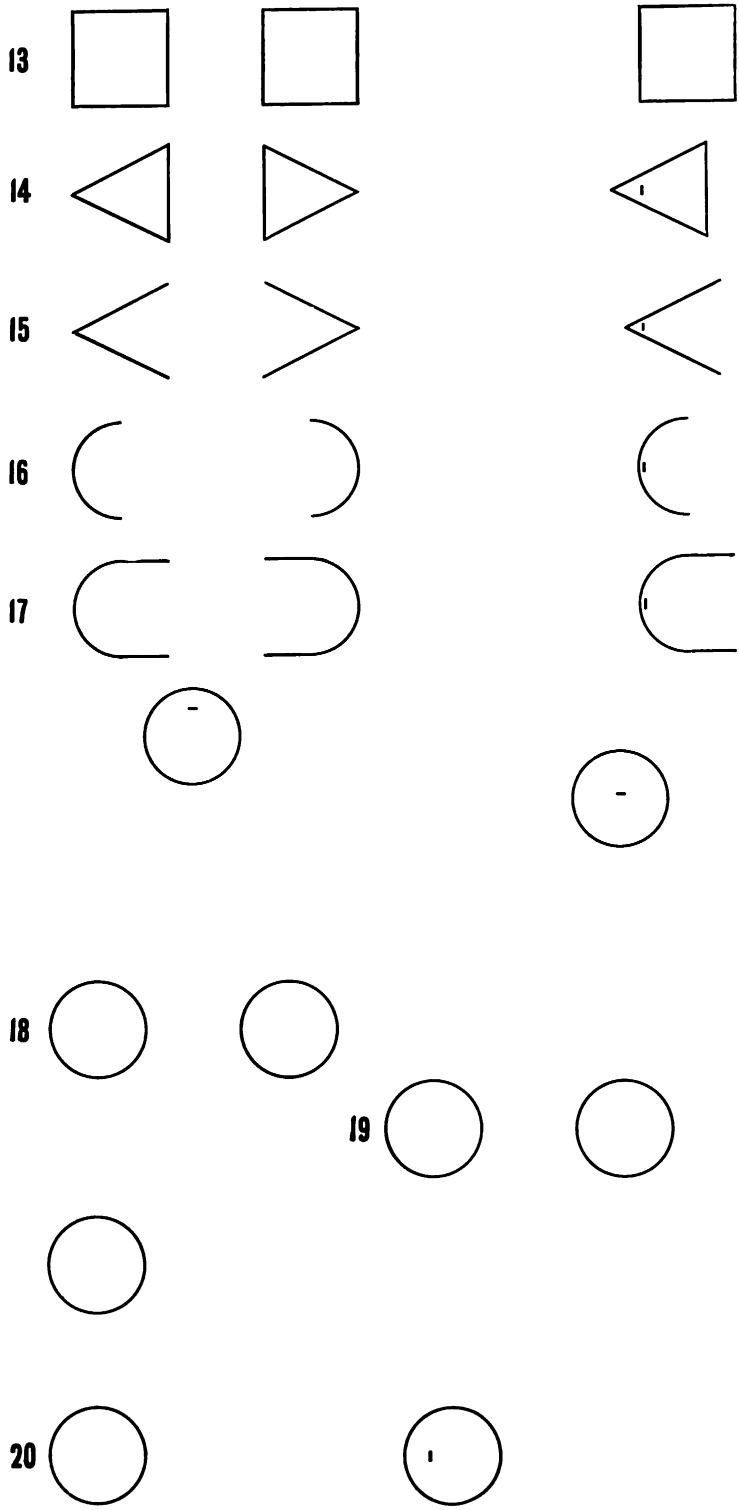
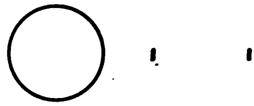


PLATE III.

21

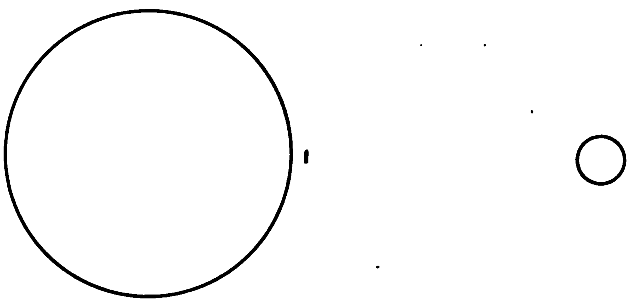


22

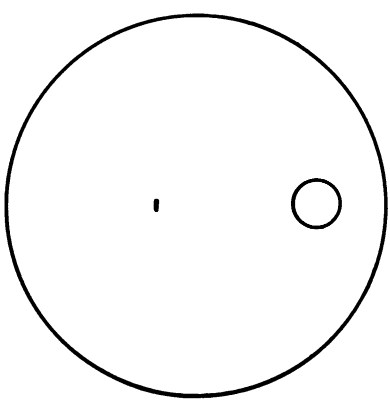
23

24

25



26



card with the rubber end of a pencil. In the trials on the forms of Type B, the bisection was made by placing the point of a triangular limb from a pair of fine compasses at the middle point. The observer was seated at a table, 70 cm. high, in such a way that he looked down upon the cards which were laid down flat. The cards were presented in order without any preparatory statement about the nature of the series. The standard member was always kept to the left. Four trials were made for each form, in the double fatigue order; i. e. two trials were made on each form in passing over the series the first time and then two more in returning over the series in the reverse order. The forms are so arranged in order as to eliminate the error of sequence as much as possible. Discussion of the illusion was evaded and no records were given out until the test had been completed.

The following conditions must be remembered in the interpretation of the results as expressed in the accompanying figures.

First, the figures are reduced in size and are therefore not supposed to bring out the original effect. Second, there are individual variations. These results are averages and do not represent fully any one of the individuals tested. Third, pointing out an illusion weakens it. In making the tests we aimed to obtain the conditions of every-day perception. The reader has the advantage of having the illusion pointed out. And, fourth, the majority of normal illusions cannot be discovered by unaided observation; they must be discovered by experiment, just as the micro-organism must be found with a microscope. They are nevertheless there and are stern realities. The proper way to verify these records is to make the test upon a person to whom the illusion has not been explained. Forms 6 and 18 are convenient for this purpose. The prime condition in the study of illusions is that the naive judgment shall be obtained without any disturbing suggestion or warning.

Statement of the Variations oj the Illusion in the First Series of Forms. (See Table I and Plates I, II, and III.)

The forms of the illusion in this series are so selected and arranged as to bring out the effect of variations in size, relief, attractiveness of design, presence or absence of base-line, shape of the limiting figures, number and mode of interruptions, direction of the members, mode of division, and relative position of the limiting figures. There is also a basis for observation upon the influence of practice and the difference in the force of the illusion for males and females.

1. Size of the Coins. Forms 3, 4, 5, and 6, composed respectively of dimes, quarters, half-dollars, and dollars, produce different degrees of illusion. The force of the illusion decreases with the size of the coin. The illusions are: for dimes, 12 per cent; for quarters, 13 per cent; for half-dollars, 14 per cent; and for dollars, 15 per cent of the respective standard distances.

As the accompanying reproductions of the forms are only one-third of the original size, they do not convey the original intensity of the illusion.

2. Relief and Attractive Contour. The part that relief plays in this illusion is brought out by a comparison of Forms 6 and 10. The circles in Form 6 indicate the relative size and positions of the coins, while in Form 10 the diagrams themselves constitute the form. In the former case the illusion is 15 per cent, and in the latter, 13 per cent. The relief increases the illusion by 2 per cent.

That this is not due to the attractiveness of the surface of the coin is demonstrated by a comparison of Forms 10 and 11. The design of the limiting figures in the latter is more attractive to the eye than the face of a dollar, but the illusion in this form is the same as for the plain circles, 13 per cent. The outline is clear and well defined in all the forms of this experiment. Other tests indicate that clearness in the outline does not add to the strength of the illusion. Complexity of outline, as in the design of a wall paper, increases the force of the illusion. It also appears that the fainter the outline is the more the eye strives to follow it.

TABLE I.

The Illusions in Forms 1-26.

Form	Standard	1st Estimate M	1st Estimate W	4th Estimate M	4th Estimate W	Ave. Estimate M	ad	Ave. Estimate W	ad	Ave. Illusion mm	Ave. Illusion %
1	124	115	105	117	120	117	7	112	8	9½	8
2	89	83	77	90	86	86	5	83	6	4½	5
3	54	45	44	50	50	46	3	47	5	7½	12
4	72	61	58	66	65	62	4	60	4	9	13
5	91	76	73	80	81	79	5	78	5	12½	14
6	114	97	93	104	98	100	6	94	4	17	15
7	114	97	91	103	98	99	5	94	6	18	16
8	38	46	45	45	45	47	2	44	3	7½	20
9	76	65	64	71	74	68	4	71	6	6½	9
10	114	97	90	104	103	100	5	98	7	15	13
11	114	98	96	101	101	100	5	99	5	14½	13
12	114	96	98	105	101	102	4	101	5	12½	11
13	114	112	110	114	120	113	5	113	4	1	1
14	114	101	102	103	104	100	4	102	8	13	11
15	114	106	104	108	109	107	4	106	6	6½	6
16	114	112	110	109	115	111	4	112	4	2½	2
17	114	111	112	110	115	111	4	113	5	2	2
18	114	83	78	91	87	87	3	81	6	30	26
19	114	91	88	98	95	94	5	92	4	21	18
20	114	101	99	107	101	105	5	101	5	11	10
21	38	40	37	39	38	39	1	38	1	½	1
22	57	55	54	58	57	56	2	55	2	1½	3
23	57	56	56	58	58	57	2	57	2	0	0
24	57	52	54	53	53	53	1	53	1	4	7
25	114	108	106	107	107	107	1	109	2	6	5
26	57	52	53	54	53	54	1	54	1	3	5

M, men; *W*, women.

ad, average of the mean variations. (This is the average of the variations found for each observer by taking the average of the variations of each record from the average of the four records on each point, regardless of sign).

mm, the number of millimeters by which the compared member was overestimated. This is the mean between the men's and the women's records, but there were twenty-eight male and only eighteen female observers.

%, the per cent of the standard distance that the illusion represents.

The "Average Estimate" is the average for the four trials of which the first and the fourth are recorded separately.

Compare the data of this table with the diagrams in Plates I, II, and III.

3. Introduction of a Base-line. The base-line lessens the illusion. The illusion is 2 per cent less in Form 12 than in Form 10. The line makes the distances to be compared more definite and tends to keep the eye from wandering over the outline of the limiting figures.

4. Form of the Limiting Figures. The variations are represented in the figures of Forms 10 and 13–17. The illusion is greatest for the circles (Form 10, 13 per cent) and smallest for the squares (Form 13, 1 per cent).

In Form 8 of this series it is demonstrated that the middle distance in the standard member is greatly overestimated when viewed by itself. When the distance across the whole standard member is viewed the case is reversed. The standard member is perceived to be shorter than it really is, not so much on account of the underestimation of the horizontal diameters of the circles as on account of the generous contraction of the middle space. This paradox is not involved in Forms 13, 14, and 15, where the suggested form of the middle space is a square. It seems strange that the strongest illusion should appear in the form that presents such conflicting conditions as Form 10.

At first sight it would appear that there is practically no illusion in Form 13, and it is generally so stated. The case is an interesting one because it is typical of what we find in many other conditions that are generally overlooked. The relative absence of any illusory effect is due to the conflict of two or more illusions which tend to cancel each other. The width of the space between the standard members is underestimated because the upper and lower limiting lines are absent and the eye sweeps freely out over the adjoining space with the result that the width of this space is perceived as the distance across the neck of a dumb-bell figure. The conditions are similar for the long space constituting the second member. It appears to be shorter than it really is, while the corresponding distance in Form 10 appears to be longer than it really is. And the standard member in the square-form appears to be longer than the standard member in the circle-

form. Therefore in placing the third square, there should be a tendency to place it too far out. As a matter of fact this does not occur, but it is placed a trifle too far in. The conditions are quite complicated. There are probably four illusions involved. These are arranged in pairs and the two opposing pairs practically balance each other. On the one side is the tendency to underestimate the distance through the open space in the second member. This is represented by (1) the tendency just stated as due to the absence of horizontal limiting lines, and (2) the well known tendency to underestimate open space as compared with filled or interrupted space. Both of these are undoubtedly present. On the other hand these are counteracted by (1) a vague effect of the Müller-Lyer illusion (Perhaps the eye sweeps over the body of the figure instead of along the limiting lines.), and (2) the tendency to overestimate a long distance which is compared with a short distance as in the comparison of the two sides of a double square.[1] To illustrate the last case, which may be doubtful, the perception of the linear distance in question depends upon the conception taken of the area which it runs through. If this area is thought of as a dumb-bell figure, the distance will be underestimated; but if it be thought of as a

[1] The reversal of the tendency to overestimate the vertical under certain conditions was discovered in some of our experiments in another investigation. A wide, white celluloid frame was made to surround a pink colored surface 101 mm. wide and 270 mm. long. The length of this area could be readily changed by pushing in white celluloid slides from the ends. The frame was placed in a horizontal position, at arms-length from the observer's eye, and perpendicular to the line of vision. The observers, thirty-seven males and twenty-five females, were students of psychology who had not yet studied illusions. They were first asked to form a perfect square. Seven made it right, within 1 mm., eighteen made the horizontal measurement too short by an average of 3 mm., and thirty-seven made the horizontal too long by an average of 5 mm. The total average overestimation of the horizontal is 2 per cent of the standard. They were then asked to form a double square, longer horizontally. Forty-eight made the horizontal line too short, by an average of 15 mm. (mean variation, 8); and fourteen made it too long, by an average of 10 mm. (mean variation, 6). On the whole the horizontal distance was made 4½ per cent *too short*. (See same for children, below.)

rectangle determined by the limiting squares, the distance will be overestimated. These two tendencies are then reciprocal.

The illusion for the triangle-form is 11 per cent, which is 2 per cent less than the illusion for the circle-form. It would seem that the directing force of the triangle should be greater than that of the circle. This is apparent to every observer, and becomes a plain cause for conscious or semi-conscious reaction. Most of the observers remarked that the sharp angles are very delusive. No one gave evidence of having an equal apprehension in regard to the circles. This apprehension of strong illusion may be even greater for Form 15, where the illusion amounts to 6 per cent only. However, when compared directly as in Plate II, the standard member in Form 14 appears to be as much shorter than the standard member in Form 15 as is necessary in order to account for the difference in the illusion. The difference in the apparent length of the standard members obtains also when base-lines are added in these two forms.

There is an illusion of only 2 per cent in Form 16. The extension of the semicircles in Form 17 does not change the illusion.

5. Direction of the Members. A vertical distance is overestimated when compared with a horizontal distance. In Forms 18, 19, and 20, this illusion is combined with the Müller-Lyer illusion. The resulting illusion is strongest in Form 18, where it amounts to 26 per cent.

In Form 20 the tendency to overestimate the length of the standard member on account of its vertical direction counteracts the Müller-Lyer illusion, making the resultant 5 per cent less than the simple Müller-Lyer illusion as in Form 6.

But in Form 19 the Müller-Lyer illusion is re-enforced by the illusion of the vertical in the second member. The resultant illusion is 3 per cent greater than the single Müller-Lyer illusion as in Form 6.

When a line compared with a horizontal line inclines from the vertical position, the overestimation of this line decreases with the increase of the amount of inclination. If the inclina-

tion of the compared member were the only condition for variation of the illusion in Forms 18 and 19, the illusion should be weaker in the former, where the member slants. But it is much greater. There are probably two new elements that contribute toward the total illusion in Form 18. There is a double estimation to be performed, and the position of the third dollar suggests the bisection of the standard member.[1] On account of the middle position of the third dollar, there is a tendency to converge all eye movements toward a point just above the middle of the horizontal line that joins the diameters of the two dollars. In so far as this takes place, it introduces the effect of bisecting the horizontal distance. The two-fold estimation requires greater effort than the single comparison and it is probable that the increase in effort also tends to strengthen the illusion.

6. Number of Interruptions. The illusion of filled as compared with empty space probably enters into every form of the Müller-Lyer illusion. As a general rule filled space or interrupted space seems greater than open space, but the rule suffers many apparent exceptions. The exception may be due to a counteracting or to an overshadowing illusion. In Forms 1 and 2 the overestimation of the interrupted and filled space is increased by the insertion of more coins in the standard member than are required for the regular Müller-Lyer figure. The Müller-Lyer illusion is thereby weakened. The standard distance is increased without increasing the span of the limiting figures. The recorded illusion for these two forms represents the Müller-Lyer illusion minus the illu-

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[1] Anticipating the need of a special explanation of the extraordinary force of the illusion in Form 18, I inserted an extra test in this series. It came between Forms 20 and 21 in order. A horizontal line was drawn 114 mm. long, and a long perpendicular was erected at the middle of this. The observer was directed to cover the upper part of the vertical line with a strip of paper so far that the exposed part of the vertical line should appear to be equal to the whole length of the horizontal. The average estimate of the whole class makes the vertical line 9 per cent too short. This is a double illusion. We shall not be far wrong if we attribute one half of it to the comparison of a vertical line with a horizontal and the other half to the bisection of the horizontal.

sion of filled space. This is 5 per cent for Form 1, and 8 per cent for Form 2, as compared with 12 per cent for Form 3. Theoretically the resulting illusion should have been smaller for the five dimes than for the four. This reversal is due to a constant tendency found in all our experiments upon normal illusions, namely, the tendency to give the illusion in the first trial fuller sway. There is, of course, some margin for chance variations. With the introduction of more coins in the standard member, the ratio between the two illusions would undoubtedly vary in a series so that the illusion of filled space would soon be the stronger.

In the following forms of this section it was found more convenient to use dollars than dimes. The variation between Forms 6 and 7 is practically immaterial, the illusion in 7 being only 1 per cent greater than in 6. Psychologically these two forms are alike, but Form 6 is more convenient and the conditions for comparison are there more favorable.

The elimination of the middle space in the standard member of Form 9 changes the proportions of the form, makes the standard more definite, and facilitates the perception of the compared distance. These conditions coöperate in reducing the illusion from 15 per cent in Form 6, to 9 per cent in Form 9.

This effect becomes quite paradoxical in view of the fact that the eliminated distance is greatly overestimated. This is demonstrated in Form 8. The compared member involves no illusion by the method employed, therefore the 20 per cent illusion expresses the actual overestimation of the middle distance of the standard member in all these circle-forms.

It would be natural to look in Form 21 for the reciprocal of the effect of the middle space in Form 8. But in Form 21 there is no illusion. It is generally known that the effect of diverging angle-lines is greater than the effect of converging angle-lines. The influence of the edges of the coins is also reduced because these are arcs of a circle, a perfect figure. The diameter of a circle is not elastic under the illusory influences.

The consequences of placing an inelastic figure in juxtaposition with an elastic figure will be noticed later.

Thinking that this non-elasticity found in Form 21 was due to the relief or to some special associations with the coins, we made some trials with a plain circle which was of the same diameter as the dollar. But the diameter of the circle is also estimated correctly by the same method.

7. *Mode of Division.* The second type of the illusion is represented in Forms 22–26. In the first three the shape of the limiting figures is varied. The dollars produce an illusion of 3 per cent, the squares apparently no illusion, and the angle-lines an illusion of 7 per cent.

The conditions for the perception and comparison of distances are here essentially different from those in the previous type. The middle space is overestimated in Form 22, and this tends to counteract the illusion. The converging forces of the two sides of the left-hand dollar nearly balance each other and the illusion depends mainly upon the inner side of the right-hand dollar.

The middle distance in Form 23 may be underestimated or overestimated according as the middle space is considered as unlimited or limited by parallel lines above and below. The Müller-Lyer illusion is probably not involved here at all, because the sections of the divided distances are so small in comparison with the size of the squares.

In Form 24 the force of the angle-line illusion is increased by the relative shortness of the standard distance. Shortening of the standard distance has the reverse effect in Form 22.

The size of the end-figures is next varied. In Form 25 the middle point is placed 5 per cent too near the small circle. The distance between the two circles is overestimated. In making the comparison the diameter of the circle is first compared with the whole distance between the two circles and then the apparent difference is distributed. No one placed the middle point inside of the large circle. If a direct comparison were made as in the previous forms, the right-hand edge of the large circle would not be clearly apperceived, and an element of contrast would also enter.

The illusion persists with equal force when the small circle is placed inside a large circle as in Form 26. The apparent middle point is placed 5 per cent of the standard distance too near the small circle. Here the illusion is due to the combined angle-line effect and the contrast of the limiting arcs.

Practice. The influence of practice is eliminated as far as possible in this series. It was, however, unavoidable in two respects, namely, (1) through the four trials upon each form, and (2) through the succession of twenty-six similar forms. The difference between the estimates for the first and the fourth trials may be seen in the table. Much of the decrease in the average of the last estimate for the class is due to a rather excessive reaction or a few observers. The illusion is strongest at first glance, before the observer has become fully orientated. The most important element in the practice is the gaining of knowledge about the presence and force of the illusion. The effect of practice is evidently the same here as in illusions of weight,[1] for which I have proved that so long as the observer remains ignorant in regard to the presence of the illusion, it will not decrease by practice. Practice tends only to make it more uniform.

Variation with Sex. Compare the average estimate for men and for women. The men are nearer right in the greater number of cases and by the greatest amounts.

Compare the records for men and for women in the first and the fourth estimate. There is a greater difference between the first and the fourth estimate for women than for men. This reaction is also a sign of suggestibility. The women start with a strong illusion and then respond to the growing suggestion that some allowance should be made.

Compare also the average mean variations. The men have the smaller mean variation. The variations with sex are all small and would be insignificant were it not that they seem to be fairly constant.

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[1] Stud. Yale Psych. Lab., 1895, III. 1.

Second Series: Influence of the Magnitude of the Angles and the Length of the Angle-lines.

In our preliminary work we repeated a part of Heymans' experiments[1] upon the influence of variations in angles and the length of the angle-lines. These tests were made in connection with the study of the formation of the square and the double square. (See note p. 19.) A simplified form of Heymans' apparatus was obtained by drawing one member of the figure on each end of the slides in the frame employed in the formation of the squares. The slide that went under the other did not have more than one end-figure, as the middle angle-lines were common to both members.[2] The adjustment was made by pushing in one slide until the two members appeared to be equal. The forms were all of the same type, being the double Müller-Lyer figure, composed of a base-line and straight angle-lines. (Form 15 with base-line.)

Thirty-one male and twenty-six female students were tested; only one trial was made on each point. In the records of the males and females, there is no difference that is worth recording; the average for all is therefore stated.

The constant base-line was 95 mm. Calling the angles A and the angle-lines L, the variations in the figures and the results may be stated as follows:

(1) A 30°, L 30 mm., estimate 75 mm., illusion 21 per cent.
(2) A 15°, L 30 mm., estimate 73 mm., illusion 22 per cent.
(3) A 60°, L 30 mm., estimate 79 mm., illusion 16 per cent.
(4) A 30°, L 45 mm., estimate 78 mm., illusion 18 per cent.
(5) A 30°, L 15 mm., estimate 81 mm., illusion 15 per cent.

As far as they extend, these results agree with those found by Heymans. The illusion is strongest with the angle of 15°,

[1] HEYMANS, *Quantitative Untersuchungen über das optische Paradoxon*, Zeitschr. f. Psychol. u. Physiol. d. Sinn., 1895, IX. 221.

[2] Placing the joint of the slides at the vertex of the middle angle, as Heymans has it, cancels a part of the influence of that angle. It is better to place one-third of the adjustable member upon the slide that contains the standard member.

and for angle-lines of 30 mm. It is remarkable that the illusion here found is no stronger than the illusion found for the trained observer by Heymans. Yet the illusion in this series is stronger than the illusion in the preceding series. This difference is due partly to the difference in knowledge or suspicions about the illusion, and partly to the smaller number of trials. (Compare the first of these forms with Forms 12 and 15 in the first series.) The tests were not made in the order in which they are described here; this "Second Series" was the first general test upon visual illusions made in this laboratory. The "First Series" of tests were made a year later and, although they were made upon a different class, many of the observers were prejudiced on account of information received from the foregoing class. While this test is not so reliable as the other, I think that it well represents the force of the illusion in the naive state of mind, when a single, unbiased estimate is made under its influence.

Third Series: Elasticity of the Apparent Distance through Open Space.

Comparison of the results obtained in the tests with Forms 8 and 21 in the first series brings out the fact that the illusion of a distance through open space has free sway, and that it is checked in a distance through a figure of known form. According to the results for Forms 19 and 20 above, the mean overestimation of the vertical is 4 per cent of the standard distance. That is a probable amount for the total overestimation of the whole vertical member regardless of any other illusion. If this error were evenly distributed among the three sections of the standard member in Form 19, the circles should appear ellipsoid, with a longer vertical axis. This they do upon very close examination but, in the ordinary methods of estimating, the circles do not appear to be distorted, and yet the overestimation of the vertical takes place. If this is true the whole distortion is forced into the distance through

the space between the circles which is unlimited laterally. That would make the illusion of the vertical, that is admitted through the middle distance in Form 19, about 12 per cent, which is too much for the simple illusion of the vertical.

If four circles of the same diameter are arranged in a rectangular form so that the vertical and the horizontal distances between the circles are equal to the diameter of a circle, the elasticity of the middle distance may be seen quite readily. From the foregoing we know that both the vertical and the horizontal members appear to be shorter than they really are (counting a member to consist of the two circles and the intervening space). The vertical members appear to be longer than the horizontal members. The circles do not appear to be distorted unless the distortion is looked for specially. When a vertical middle space is compared directly with a horizontal middle space the former appears to be the longer, and the difference is at least twice as apparent as the difference between the whole members. And, if the vertical and the horizontal middle distances are compared indirectly, in terms or by means of the diameter of the intervening circle, the difference becomes still greater.

The following is an attempt to determine this difference quantitatively. Two sets of twelve cards each were made exactly alike. Each card contained two circles, 10 cm. in diameter, and the distances between the circles varied in a series of 1 cm. steps from 5 cm. to 16 cm., inclusive. The cards of one set were placed in a vertical position and those of the other in a horizontal position. They stood upon an easel, at right angles to the line of vision and three meters from the observer. To avoid, as far as possible, the error of succession,[1] they were arranged in the following irregular order (where h denotes horizontal, and v, vertical, and the middle distances are given in centimeters): 16 v, 15 v, 6 h, 5 h, 10 h,

[1] The test began with two vertical figures, and it was not announced beforehand that there would be any horizontal figures. This precaution was taken because the method of estimating that is followed in the first trials is liable to be continued.

9 h, 5 v, 6 v, 9 v, 10 v, 14 h, 13 h, 14 v, 13 v, 7 v, 8 v, 12 h, 11 h, 8 h, 7 h, 12 v, 11 v, 16 h, and 15 h. The cards were 57 cm. long and 36 cm. wide and had a light blue tint. The lines were 1 mm. wide.

This test was made upon twenty-four of the gentlemen who had taken part in the First Series. They were aware of the disturbing influence of the interrupting lines, but they were requested not to make any theoretical allowance for that; e. g., if they surmised that there might be an illusory effect, of say 10 per cent, they should not take that into account but estimate the distances as they actually appeared upon the very closest inspection. These are the particular instructions: Consider the diameter of a circle to consist of ten units. How many such units are there in the distance between the two circles? They had only one trial on each point. The records are contained in Table II.

TABLE II.

Forced Overestimation of the Vertical.

St	*HE*	*d*	*VE*	*d*	%*HI*	%*VI*	%*VI*-%*HI*
5	5.3	0.8	5.7	1.0	6	14	8
6	6.5	0.8	7.0	1.0	8	16	8
7	7.0	0.7	7.8	0.9	0	11	11
8	8.0	0.7	9.0	0.9	0	13	13
9	9.6	1.2	10.7	1.3	7	19	12
10	10.7	1.3	11.8	1.1	7	18	11
11	11.5	0.9	12.6	1.0	5	15	10
12	12.6	0.7	13.1	1.2	5	9	4
13	13.6	1.0	14.6	1.6	4	12	8
14	14.5	1.8	15.9	1.5	4	14	10
15	15.7	1.2	17.6	2.0	5	17	12
16	16.3	1.5	18.7	2.1	2	17	15
				Average	4	15	11

St, standard distance between the circles.
HE, average estimate of the horizontal distance.
VE, average estimate of the vertical distance.
d, mean variation of the records for all the observers.
%*HI*, per cent of overestimation for the horizontal.
%*VI*, per cent of overestimation for the vertical.
The unit of measurement is one centimeter.

The data are not sufficiently numerous to enable us to establish any law for the variation with the length of the middle distance. As the standard distance, 10 cm., is in the middle of the series and there is no regular variation, it may be permissible to apply the average of the illusions for all the distances to that one for convenience in a crude comparison. The horizontal middle distances are overestimated by 4 per cent, on the average; the vertical, by 15 per cent. The difference between these two, 11 per cent, is due to the difference in direction. This is not the ordinary overestimation of the vertical because the whole figure was in a vertical position.

The figure in Plate IV is drawn according to the data obtained in this test. To the persons that were tested this figure should appear perfect in arrangement; i. e., the middle distances should appear to be equal to the diameter of a circle. The horizontal middle distance is 4 per cent shorter than the standard, and the vertical middle distance is 15 per cent shorter than the standard distance. Of course, no two persons will see it alike and much depends upon the relative size of the figure, its distance from the eye of the observer, and the fact that the illusion is pointed out.

In making the comparison for the vertical figure there seems to be a tendency to estimate the horizontal diameter of the circle first and then assume that, as the circle is a perfect figure, this is also the vertical diameter. The middle distance is therefore virtually compared with the horizontal diameter in the perception of which there is perhaps no illusion (See Form 21 above). There is then no overestimation of the supposed vertical diameter of the circle but there is nothing to check the overestimation of the vertical middle distance. This may account for a normal overestimation of the middle distance, but 11 per cent is more than the normal overestimation of the vertical. The excess may be accounted for by the combination of the illusion of the vertical with the illusion of angle-lines, upon the hypothesis that the illusion of the vertical is augmented by increased effort in the estimation. It requires greater effort in estimating the length of the middle

distance in this figure than in estimating the length of a plain vertical line.

The illusion is inhibited in the circles because we know that the circle is a perfect figure. Any other figure which possesses an equal symmetry will produce a similar effect. If squares are substituted for the circles, the diagonals of the squares lying in the same straight line, the effect is even greater than for the circles, on account of the decreased limitation of the middle space.

A visual illusion of distance perhaps occurs wherever lines, real or imaginary, are interrupted by lines forming angles other than right-angles with the base-line, but in most cases it is difficult to determine it on account of the want of a suitable unit of measurement. A simple case is that in which distances appear to stand in the ratio of 1:1, as in a single vertical or horizontal member in Plate IV. It is a favorite type of designs in wall papers, carpets, silks, etc. The section of a design on a wall paper frieze seen in Plate V is an illustration. The horizontal diameter of the incomplete circles in the upper part of the figure is 125 mm. and the distance between the figures is 112 mm. in the original. These distances appear to be equal. One of the most pleasing effects is the ratio of 1:1. This is obtained by making proper allowance for the illusion. To show how common this apparent ratio is, I may mention that I have found, in the same room, more or less complex patterns of this type on the wall papers, the table cloth, and the carpets. The middle spaces were approximately nine-tenths as long as the diameter of the limiting circular figures, but they appeared to be equal.

The same forces that cause the Müller-Lyer illusion upon plane surfaces also influence the perception of geometric forms. To mention a homely illustration, the difference between the height and the diameter of a flour barrel appears to be much greater than it really is. In any cylinder whose length is equal to its diameter the length appears to be decidedly greater than the diameter, especially if the actual proportions are not known. The illusion is still greater if the

PLATE IV.

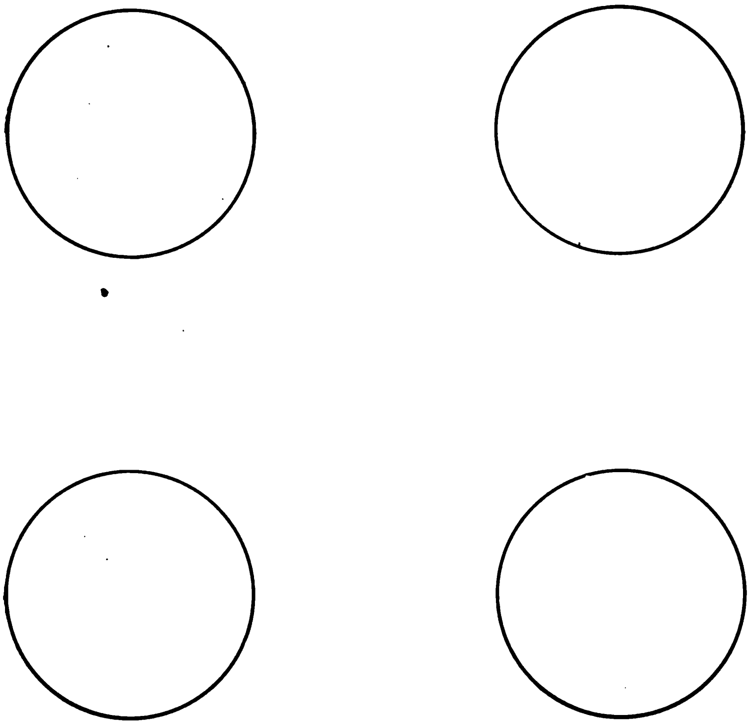

PLATE V.

length is actually greater than the diameter. Much of course depends upon the point of view and combinations with other illusions. The problem is now being investigated. This illusion is an extremely important factor in the æsthetics of geometric forms.

Fourth Series: Variation with Mental Development.

(These tests were performed by Miss Eva M. White.)

The following tests were made upon school children of the ages from six to fifteen, inclusive. (See description of method, p. 3, above.) The results are contained in Table III. The headings of the columns in the table refer to the corresponding paragraphs in the following statement.

A. The Müller-Lyer illusion with three silver dollars as in Form 6 of the First Series. The illusion amounts to 31 per cent for girls and 30 per cent for boys. It is 15 per cent for the university students.

B. The conflict of the Müller-Lyer illusion with the illusion of filled space. This is like Form 1 of the First Series (p. 6), but instead of dimes brass disks were here used. The standard member consisted of five disks which, when arranged like the dimes in Form 1, made the standard member 114 mm. The disks were fixed on a wire which also formed a base-line. A sixth disk was adjustable like the fifth dime in Form 1. The illusion amounts to 11 per cent for the girls and 15 per cent for the boys. Only a general comparison of this can be made with the 8 per cent illusion for the university students in Form 1.

C. The Müller-Lyer illusion—the same as the first form described on p. 25. (Base-line 95 mm., angles 30°, angle-lines 30 mm.) The same apparatus was used as in the test with the students. The illusion amounts to 22 per cent for the girls and 21 per cent for the boys. This happens to be exactly the same as was found for the students in that test. The students had more nearly the same conditions as the children in this test than in any other. I consider the comparison much fairer in this case than in the case of Form A.

TABLE III.

Force of the Normal Illusions of Sight in Children.

GIRLS.

Age	*n*	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*E*	*d*
6	10	90	28	107	12	80	14	86	6	141	25
7	9	72	21	94	8	76	7	84	8	148	16
8	10	75	14	103	8	76	4	84	6	157	19
9	9	66	11	98	8	79	8	83	6	177	15
10	9	78	7	99	6	67	3	83	4	180	7
11	10	68	10	104	6	73	5	85	7	180	16
12	10	77	10	102	6	74	10	84	4	178	10
13	10	84	7	104	6	74	10	84	4	178	10
14	10	87	5	103	4	73	9	86	4	183	13
15	7	90	8	106	6	74	6	88	5	189	13
Average		79	12	102	7	74	7	85	6	171	14
Standard		114		114		95		101		202	

BOYS.

Age	*n*	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*E*	*d*
6	9	76	18	89	20	74	9	82	14	147	21
7	10	74	23	95	8	81	13	86	6	161	23
8	9	65	11	97	6	80	5	87	5	145	32
9	7	69	17	89	15	71	5	85	7	171	17
10	6	90	12	96	5	80	6	87	6	187	13
11	10	85	9	99	5	71	6	84	5	177	11
12	10	79	9	96	4	68	5	82	4	180	9
13	8	87	5	106	8	68	7	88	4	184	7
14	9	90	11	100	11	73	6	86	9	190	20
15	6	80	3	97	3	85	12	89	6	191	10
Average		80	11	97	9	75	7	86	7	173	16

n, number of children tested.
d, mean variation.
Unit of measurement, the millimeter.
The forms are designated by the capitals as in the text.

D. The vertical, bisecting-line illusion. A perpendicular 125 mm. long was erected at the middle of a horizontal line 101 mm. long. The test consisted in making the two lines appear equal. The adjustment was made by passing a card down over the upper part of the vertical. The illusion amounts to 18 per cent for the girls and 17 per cent for the boys. This may be compared with the same test made, though with a standard of 114 mm., upon the students (p. 21) in which the illusion averaged 9 per cent. The comparison is not direct because the students had good grounds for supposing that there was an illusion, but the children had not.

E. The double square. The method and apparatus described on p. 19 were employed. The girls make the apparent double square 9 per cent too short, and the boys make it 8 per cent too short. The average for the students is 4 ½ per cent. Here the comparison between the children and the students is perfectly legitimate. The records for the children clearly confirm the principle that the overestimation of the vertical depends upon the ratio between the horizontal and the vertical. For this illusion there is a decided improvement with age in the children. The improvement of the students over the children is also certain.

The illusion has practically the same force for girls and boys. For the first four forms of the illusion there is no decrease in the illusion with development of the children. The only improvement is in uniformity, which is seen in the mean variation. In comparing the records of the students and the children in Forms A, B, and D, it must be borne in mind that the superiority of the students is not all in greater accuracy of perception, but often in the possession of some sort of advantageous caution against illusion.

These data upon the illusions in children were collated and compared with the classification of the children according to "general mental ability" by the method that is illustrated for hearing-ability in Table VII. The comparison reveals no constant tendency for the illusion to vary with the "brightness" of the children.

II. The Material-weight Illusion.

A few years ago I called attention to the existence of a negative illusion of weight which is due to the appearance of the material in the object that is lifted. The original statement[1] was made upon the basis of a small number of experiments. I have here undertaken to verify those observations and to study the variations in the illusion for age and sex. I suggest the name "material-weight" for this illusion because that coördinates it with the "size-weight" illusion and indicates its nature.

First Series: Illusion Unknown to Observers.

Three blocks were made of wood, iron, and cork, respectively. They were all of the same size, cylindrical, 31 mm. in diameter, and 40 mm. long. They were also of a uniform weight, 55 g. The wood and the cork had been filled and the iron had been made hollow. A standard set of blocks to be used as a means of measurement was made of hard rubber and polished. The standard blocks varied in weight from 15 g. to 90 g., by five-gram steps. But they were of the same size and shape as the other blocks.

The size-weight illusion was introduced in the same series in order to obtain a comparison of the new illusion with this well known illusion. Two blocks like the blocks of the standard series in every respect except in length and weight were used. They were of the same weight, 55 g., but one was 12 mm. long and the other was 140 mm. long. They may be designated as the small and the large.

The blocks were kept behind a screen and were handed to the observer in order. The wooden block was first presented together with the 55 g. block from the standard series and the observer was requested to compare them according to the following written directions.

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[1] Stud. Yale Psych. Lab., 1895, III. 18.

"*Test of discriminative sensibility for weight.* Compare the weight of the presented blocks by lifting them two or three times in rapid succession, being careful always to keep the blocks in the same position, grasp them in the same manner, lift them about one inch at a uniform speed, and replace them gently and quickly. State whether they are equal or different in weight and if different state which is the heavier."

If as usual the wood was pronounced heavier, heavier standard blocks were presented in order until the block from the standard series had been pronounced heavier in two consecutive judgments. Then lighter standard blocks were presented in order until they had been pronounced lighter in two consecutive judgments. This method was pursued until four upper and four lower determinations of the region of equality had been made. In the case of a few persons who were very slow in making the comparisons, only two such complete determinations could be made. The same mode of procedure was followed for the iron and the cork blocks. The same method was also followed for the large and the small blocks, except that the 65 g. block was handed first with the small block, and the 45 g. block first with the large block, in order to save time. This method is somewhat arbitrary but it is time-saving and excludes the danger of suggestion from the observer as the order of the blocks depends upon the observer's judgments. It is entirely satisfactory. The trials were made in the double fatigue order. In the statement of the results, the mean between the upper and the lower limits of the estimated weight is taken as the apparent weight of the block that has been tried. The mean variation was obtained by taking the average of the variations of the individual upper and lower limits from the average of their means.

I place the entire table of the results of these measurements on record in order to establish the fact of the existence of the illusion, and show its extent and individual variations. The cork and the wooden blocks are overestimated and the iron block is underestimated. The essential condition of the illu-

sion is that the preliminary estimate of the weight of the object shall be wrong. In this case that is brought about by making the objects appear to be solid and made of materials of different weight. Before lifting an object we normally estimate the approximate weight by sight, and the effort to be exerted in lifting is adjusted semi-automatically upon the

TABLE IV. (*A*). *Men.*

The Material-weight and the Size-weight Illusions When They are Unknown to the Observers.

N	*Wood*	*d*	*Iron*	*d*	*Cork*	*d*	*Small*	*d*	*Large*	*d*
2	55	3	57	5	61	3	70	2	48	0
4	57	1	54	2	59	3	76	4	35	0
6	61	2	49	2	58	0	58	0	45	2
8	63	3	46	2	60	4	69	1	24	6
10	66	1	44	4	62	3	73	2	31	3
12	56	2	55	3	59	1	61	2	48	0
16	59	4	46	4	58	9	71	10	37	2
18	65	2	46	4	61	5	83	0	31	4
20	55	4	55	3	64	2	75	3	38	0
22	63	5	45	5	63	0	68	0	50	3
24	61	1	48	0	65	3	66	1	43	1
26	56	2	50	3	58	2	58	5	53	0
28	58	0	48	0	61	3	73	0	45	3
30	63	0	43	5	68	0	83	0	30	3
32	57	2	58	1	58	2	69	4	44	1
34	58	2	46	3	55	3	66	1	45	6
36	62	1	48	0	58	5	90	0	33	3
38	64	3	47	2	60	1	64	2	41	6
40	57	2	49	2	55	2	80	2	35	2
42	64	0	55	2	68	0	82	7	48	0
44	60	2	50	1	63	2	70	2	54	3
46	63	0	50	2	58	3	73	0	40	2
48	58	3	53	0	59	3	69	2	43	5
50	62	3	49	1	57	2	77	2	43	0
52	53	0	51	1	55	2	78	0	35	0
54	60	2	48	5	60	2	72	0	40	2
56	60	2	49	1	58	1	86	1	30	0
58	60	2	53	2	55	3	62	1	45	5
Average	60	2	50	2	60	3	72	2	41	2

The measurement is in grams. The standard is 55 g.
N, the observers by number.
d, mean variation.

TABLE IV. Continued. (*B*). *Women*.

N	*Wood*	*d*	*Iron*	*d*	*Cork*	*d*	*Small*	*d*	*Large*	*d*
1	56	2	51	4	65	2	70	4	33	3
5	59	2	50	0	63	2	82	4	35	0
7	63	5	51	2	63	4	73	5	38	5
9	64	9	47	3	65	3	79	8	34	2
11	58	0	45	2	62	1	80	2	39	4
17	58	0	49	6	65	2	83	4	30	2
19	55	3	54	1	54	1	70	3	35	4
21	62	4	48	3	64	2	81	2	31	1
23	57	1	51	3	61	1	86	1	30	3
25	39	1	46	3	59	1	78	7	39	3
27	59	3	50	1	59	4	73	3	34	1
29	50	1	53	5	66	3	78	4	36	1
31	59	3	50	4	68	4	83	2	35	0
33	65	8	47	2	57	1	85	4	45	4
35	60	3	51	3	56	5	76	1	33	2
37	58	0	49	6	59	1	83	6	39	3
39	57	3	53	2	64	2	80	0	29	1
Average	59	3	49	3	62	2	79	4	35	2

basis of this preliminary estimate. If insufficient effort is put forth at the beginning of the lifting the weight of the object will be overestimated. If too great effort is put forth the weight of the object will be underestimated.

It is not necessary to state the illusion in relative terms of the standard series. As this is a common scale having the same unit of measurement as the illusion blocks, it may be eliminated and the results may be expressed as an absolute difference in the apparent weight of two illusion blocks. This has been demonstrated for the size-weight illusion[1] and the principle of the measurement is the same. Thus, the average estimate by the men indicates that the cork and the wooden blocks each seem to weigh 10 g. more than the iron block although they are actually the same weight. The illusion is 18 per cent of the actual weight. The women estimate the difference between the wooden and the iron blocks the same as the men, but they estimate the difference between the cork and the

[1] SEASHORE, *Weber's Law in Illusions*, Stud. Yale Psych. Lab., 1896, IV, 62.

iron blocks to be 13 g. which is 24 per cent of the total weight.

These results may be compared with the results for the size-weight illusion which are contained in the same table. The comparison is necessarily relative to these particular sizes, materials, etc. In a general way it is apparent that the material-weight illusion is not as strong as the size-weight illusion, but practically as constant.

The illusion is not limited to these particular artificial conditions. It occurs whenever the appearance of the material which constitutes the object leads us to think that the object is heavier or lighter than it really is. The erroneous estimate may be due to false appearance of the material, erroneous associations of weight, or failure to acquire a true conception of the normal weight of the material. Thus, if a breakfast roll is heavier than it appears to be when looked at, we judge it to be still heavier than it actually is when we lift it. The weight of a metal tube is ordinarily underestimated, not only because it is larger than a solid piece but also because it is associated with a solid rod of the same material and dimensions. The weight of aluminium is generally underestimated and the weight of mercury is generally overestimated because we are very slow in learning that these are actually so different from other metals. It is important to notice that no one thought that the blocks in this test were actually solid, of a uniform material. It is at once apparent that the cork block is too heavy to be all cork and the iron block too light to be solid iron. After the first trial the observers knew what to expect but the illusion did not disappear. This shows that the illusion rests upon a subliminal or automatic process which tends to continue when once established, despite the opposing knowledge.

The best direct demonstration of the cause of the illusion and its persistence is obtained by lifting the blocks gently and observing that although the cork has its natural roughness and the iron has a polished surface the cork generally tends to slip, as the iron does not. Although we know all about the conditions, we automatically grasp the iron with greater force than we grasp the cork.

This test was made before the subject had been discussed in the class. A few of the observers may have been aware of the illusion and it is possible that they may have made conscious or unconscious corrections. Upon inquiry it was found that no one knew the exact extent of the illusion and only a small per cent knew anything about its nature. The illusion is therefore nearly the maximum for these particular conditions.

Second Series: Illusion Known to the Observers.

(These tests were made by Miss Anna Kierulff.)

The same test was made upon the other class, with this variation that the illusion was demonstrated and fully discussed in the class before the test was made. The following was added to the above written directions: "Do not guess or make any allowance for possible errors due to the difference in the materials of the blocks." The method described above was employed.

In addition to the first three blocks a lead block similar to the iron block was also included, the object being to determine whether the illusion would be greater for lead than for iron, as it tends to be greater for cork than for wood. In this respect the test cannot be said to be successful because it was difficult to notice any difference between the lead and the iron blocks. The fact that there is only a small difference between the illusion for the wood and the cork may be taken to indicate that for many the maximum illusion is reached with wood and that additional increase in the illusory appearance fails to increase the illusion.

As the records are of the same nature as those contained in Table IV, it will suffice to quote the final averages with the corresponding averages of the mean variations, which may be designated by ad. For the twenty-nine men the records run thus: wood, 57 g., ad., 3 g.; iron, 53 g., ad., 3 g.; lead, 53 g., ad., 3 g.; and cork, 59 g., ad., 3 g. For the twenty-six women: wood, 56 g., ad., 3 g.; iron, 53 g., ad., 2 g.; lead, 53 g., ad., 2 g.; and cork, 58 g., ad., 3 g. The illusion persists even when understood in detail but is not so strong as when unknown.

After the regular trials the observers were requested to arrange the blocks in the order of their apparent weight, knowing that they were equal. Ten declared that the four blocks appeared equal. That such assertions are vague and uncertain is demonstrated by the fact that four of these ten showed more than the average illusion according to the regular measurement. The judgments of those that noticed apparent differences are exhibited in the following tabular form where the Roman numerals indicate the order of weight, beginning with the heaviest.

	I	*II*	*III*	*IV*
Cork	34	8	3	0
Wood	8	28	8	1
Iron	2	4	17	22
Lead	1	5	17	22

Third Series: Variation with Mental Development.

(*These tests were made by Mr. I. I. Dalbey.*)

Similar tests were made with the same apparatus upon the school children (see p. 3). The method was necessarily simpler and less reliable. The blocks of the standard series were arranged in a line on the table in order of weight. The children were first required to make sure that they understood this order. Then they were required to match each of the illusion blocks in weight with a block in the standard series by comparing them under the most favorable conditions. The results are given for the different ages in Table V. As there is no great difference between the girls' and the boys' records they are stated together in the body of the table and only the averages are given separately.

There is a constant tendency to select a block that is too light. This may be accounted for by the circumstance that although the children were allowed to proceed up and down the series at pleasure there was a tendency to come to a decision oftener and more readily when passing in the direction of the heavier blocks. In such cases the records may represent one limit of the region of equality instead of the middle or average. In the two foregoing series of measurements it

has been demonstrated that the cork and the wood are overestimated and the iron and the lead are underestimated, and in the study of the size-weight illusion it was shown that the results may be stated in terms of the illusion blocks instead of in the terms of the standard series. The result may therefore be stated in terms of the apparent difference between two illusion blocks. The average apparent difference between the cork and the lead block is thus 11 g., or 20 per cent of the actual weight; between the cork and the iron, the same; and between the wood and the iron or lead, 7 g., or 13

TABLE V.

The Material-weight Illusion for Children.

Age	*n*	*Wood*	*d*	*Iron*	*d*	*Lead*	*d*	*Cork*	*d*
6	19	56	10	48	7	46	10	60	13
7	19	53	9	44	10	41	8	49	8
8	19	55	6	49	7	51	7	59	8
9	16	54	5	48	5	49	7	60	6
10	18	53	3	46	5	47	8	57	7
11	20	56	6	48	4	44	5	61	5
12	20	54	3	47	7	49	7	60	6
13	18	55	3	44	6	46	4	57	7
14	19	55	4	48	5	48	6	60	5
15	13	52	4	45	6	49	4	61	7
Average		54	5	47	6	47	7	58	7
Boys		52	5	45	6	47	7	58	8
Girls		56	6	48	7	47	6	59	6

n, the number of children for each age.
d, mean variation.

per cent of the actual weight. This is virtually what was found for the university students. This illusion does not vary with age. On the whole there is no noticeable variation with sex among the children.

The children were classified according to the degree of illusion that they showed, and this classification was collated with the classification according to "general mental ability" by the method illustrated in Table IX for hearing-ability. The comparison revealed no functional relation between "brightness" and suggestibility in this particular case.

The small variation with sex that we find is significant. These results are respectfully submitted for comparison with those published by Dr. Wolfe,[1] in which he finds the size-weight illusion twice as strong for women as for men. The material-weight illusion is too mild to form a good basis for the discussion of this. The question can best be discussed on the basis of the size-weight illusion. The data in Table IV represent as fairly as we can determine the relative susceptibility of men and women to this illusion, when a careful judgment is made under the most uniform conditions.

The Transition from Negative to Positive Illusions of Weight.

The material-weight illusion here discussed is a negative illusion because it is contrary to the suggestion. The commonest illusions in all senses are positive; we realize what we expect or what is suggested. By analogy we may infer that the material-weight suggestion may also produce positive illusions. The negative illusion is produced because the suggestion is too violent to be accepted. The very reaction of the mind against the violent suggestion intensifies the attention to the sensory signs of effort which are misleading. But if the suggestion is made mild and reasonable so that it does not surprise the observer, it will be the determining element in the perception and the sensory elements will be relatively overlooked. If this hypothesis is true we may expect to find an uninterrupted gradation from the positive into various degrees of the negative illusion. Some preliminary trials on this were made upon the children in connection with the above test.

Two Florence flasks were filled, one with unground roasted coffee and the other with common white beans. Some lead was introduced into the center of the coffee and some cotton into the beans. Neither cotton nor lead appeared at the surface. The two flasks were thus reduced to a mean weight of 63 g. A

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[1] WOLFE, *The Effects of Size on Judgments of Weight*, Psych. Rev., 1898, V. 26.

similar but larger pair of flasks were filled in the same way and reduced to a mean weight of 244 g. The children were asked to compare them, by lifting, and tell which was the heavier. The experiment is based upon the general knowledge that beans are heavier than coffee. The judgments ran as follows for the small flasks: coffee heavier, 86; beans heavier, 89; and, "no difference perceived," 10. For the larger flasks the result was as follows: coffee heavier, 119; beans heavier, 59; and, "no difference perceived," 9. Thus, there is no illusion for the small flasks and for the large ones there is a balance in favor of the negative illusion.

Two cylinders were turned out of pine, 30 mm. in diameter, one 30 cm. long and the other 29 cm. long. They were reduced to a mean weight of 83 g. It was supposed that the clearly perceptible difference in size would act as a suggestion and that the illusion would be positive because the suggested difference was so small that sensory evidence to the contrary would not be detected. The children judged them as follows: long heavier, 61; short heavier, 110; and, "no difference perceived," 10. The conditions are not simple, but the negative illusion still obtains.

The "Color-weight" Illusion.

It is generally thought that dark colored objects appear to be heavier than light colored objects. If such an illusion exists it would be involved in the coffee and bean tests. A test for this was included in the Third Series. Two blocks were made of the same size and shape as the standard rubber blocks. One was painted white and the other black. They were weighted to 55 g. What material they were made of could not be seen. The children matched them in the standard series in the same way that they matched the blocks of different material. The results show no illusion.

Energy Economized by the Illusion.

In this connection it may be of interest to mention a new problem in the study of illusions of weight. While studying

the above illusion, Miss Kierulff suggested that it might be possible to increase the efficiency of the muscular effort by means of the illusion. So long as the lifter is not exerting the maximum effort, there is no doubt but what he lifts the object which seems lighter with greater ease than the one which seems heavier. What would be the effect of the illusion upon the fatigue if a man would lift a ten-pound object fifty times in succession, being fully convinced that it did not seem to weigh more than five pounds? Many conditions must be considered in answering that question, and we have no experiments, but it is probable that with a moderate weight like this for a strong man he would feel at the end of the trial more as if he had lifted five pounds than ten pounds.

But can the maximum effort be increased by the illusion? Theoretically there should be no illusion in the maximum effort. We have arranged an experimental test but have only carried it far enough to find that there are several interesting problems involved. The maximum lifting ability is tested with objects in which the conditions of the size-weight illusion are present. A flour barrel and a half-peck measure are used. Nearly all who have tried it can lift more in the barrel than in the half-peck measure. Even when the maximum weight is lifted, there is a tendency to judge the barrel to be lighter and to try to account for the failure to lift more by fatigue or unequal distribution of effort.

III. LOCALIZATION OF SOUND IN THE MEDIAN PLANE.

(These tests were made with the assistance of Miss Mabel Williams.)

The general problem was to determine some of the constant tendencies in the localization of sound.

The sound was produced by a 100 v. d. electric fork in a distant room. The fork interrupted the primary circuit of an

induction coil in whose secondary circuit three telephone receivers were inserted. One receiver (R) was placed seven feet to the right of the observer's head and another (L) seven feet to the left, both in the aural axis. The third receiver (C) was placed two feet vertically overhead. Two different intensities of the sound were used for each receiver. The fainter was just clearly perceptible, and the stronger was such that the normal ear could detect it at a distance of about one hundred feet. In the following the strong sounds are designated by capitals and the weak by small letters. The trials were always made in the following order: R, L, C, c, rl (together), RL, RL, rl, c, C, L, R, RL, rl.

Thirty-nine students of psychology whose hearing-ability had been previously measured, were experimented upon. There were twenty-five men and fourteen women. They were blindfolded before entering the experiment room and told that the position of the experimenter and the shape of the room did not determine the location of the sounds. The experimenter turned on all the sounds noiselessly, remaining seated in the same position. The students were asked to determine the distance in feet and the angular direction by degrees in the horizontal and the vertical planes. The distance was required merely as an aid in the angular localization. The variation in intensity was introduced in order to call forth renewed effort in each trial. As the influence of variation in intensity is discussed in the sequel it may be neglected in the present qualitative statement.

We may consider the results with reference to three sources of sound:

(1) The lateral sounds—those that originate in R and L acting singly;

(2) The fused median sounds—those that originate in RL and rl, i. e., the resultant of two symmetrically located, simultaneous sounds; and,

(3) The single median sounds—those that originated in C and c.

Lateral sounds can be localized with reference to two planes.

Median sounds can be localized with certainty in one plane only. It is well known that if two sounds in the positions of R and L are sounded simultaneously they are perceived as one and this resultant is located in the median plane.

1. *Is there any constant tendency to localize a median sound in any particular section of that plane?*

(a) The single median sounds.

Twenty-two persons always locate these sounds in front of a vertical plane through the aural axis, three always locate them back of this plane, and fourteen vary.

Of all these sounds, 77 per cent are located in front of the vertical plane, 20 per cent back of it, and 3 per cent in it.

Twenty-two persons always locate these sounds above the horizontal plane through the aural axis, one always below, and sixteen vary.

The same sounds are located, 78 per cent above the horizontal plane, 11 per cent below, and 11 per cent in it.

If the median plane be divided into four quadrants, by two lines that cross the vertical diameter at an angle of 45°, the same records will be distributed as follows: 52 per cent in the front quadrant, 38 per cent in the upper quadrant, 10 per cent in the back quadrant, and none in the lower quadrant.

Therefore, there is a decided tendency to locate the single median sound, that is produced directly overhead, above and in front of the ears, i. e., upward and forward.

(b) The fused median sound.

Three persons locate all of these in front of the vertical plane, eight persons locate all behind this plane, and twenty-eight vary.

Of all the fused median sounds, 25 per cent are located in front of the vertical plane, 73 per cent back of it and 3 per cent in it.

Eighteen persons locate all these sounds above the horizontal plane, one locates all in it, and twenty vary.

Of all these sounds, 72 per cent are located above the horizontal plane, 12 per cent below, and 16 per cent in it.

Thirty-nine per cent are located in the front quadrant, 21

per cent in the upper quadrant, 39 per cent in the back quadrant, and 1 per cent in the lower quadrant.

Therefore, there is a tendency to place the fused median sound, that is produced in the binaural axis, above the ears. This tendency is virtually as strong in this case as in the case of the single median sound that actually came from above.

If we consider the front and the back quadrants, we find no tendency in the fused median sound to favor either one of these. But if the vertical line through the head be considered the dividing line, there will appear to be a decided tendency to place the fused median sounds back of this.

2. *Is the median sound localized on the side of the stronger ear?*

3. *Is the lateral localization of a fused median sound as definite as the lateral localization of a single median sound?*

(a) The single median sounds.

Three groups of ten persons each were formed upon the basis of the comparative keenness of the two ears.[1] Group 1 comprises the ten who have the greatest difference in the acuteness of the two ears, Group 2 those whose ears show the next greatest difference in acuteness of hearing, and Group 3 those whose ears have nearest the same degree of acuteness.

Group 1 located 57 per cent of these sounds in the median plane, 38 per cent on the side of the stronger ear, and 5 per cent on the side of the weaker ear.

Group 2 located 45 per cent of these sounds in the median plane, 33 per cent on the side of the stronger ear, and 22 per cent on the side of the weaker ear.

Group 3 located 58 per cent in the median plane, 20 per cent to the left, and 22 per cent to the right of this plane.

Therefore, there is a tendency to locate the single median sound on the side of the stronger ear.

(b) The fused median sounds.

Group 1 located 36 per cent of the fused median sounds in

[1] For test of the comparative acuteness of hearing in the two ears, see Section IV, p. 55.

the median plane, 32 per cent on the side of the stronger ear, and 32 per cent on the side of the weaker ear.

Group 2 located 19 per cent in the median plane, 41 per cent on the side of the stronger ear, and 40 per cent on the side of the weaker ear.

Group 3 located 45 cent in the median plane, 22 per cent on the right, and 33 per cent on the left side of that plane.

Therefore, in the case of the fused median sound, a correction is made for the difference of the two ears in acuteness of hearing, or else this sound is not located laterally with sufficient definiteness to reveal small variations. It is probable that the latter is the case because the angular variation of the fused median sounds is much greater than for the single median sounds. This could of course be given quantitatively, but a qualitative statement is sufficient for the present purpose. The lateral localization of a fused median sound is less definite than the lateral localization of a single median sound.

4. *Is there any constant tendency in the misplacement of a lateral sound?*

This is determined with reference to the sounds R and L.

Six persons located all the lateral sounds above, and four located all in front of the true source. No one located all the lateral sounds back of the true source or below it.

Fifty-two per cent of the lateral sounds were located above, 23 per cent below, and 25 per cent in the true plane.

Fifty-four per cent were located in front, 31 per cent back of, and 15 per cent in the true plane.

Therefore there is a tendency to locate sounds, that lie in the aural axis, above and in front of the axis. This is like the tendency for the single median sound, but it is not quite so strong.

It is well known that it is difficult to localize sounds at all. As all the observers were untrained, there was much uncertainty exhibited in these experiments, and the results are very fluctuating. It will, however, be seen that I have based no conclusion upon any small variations.

5. *Can the unpracticed observer localize the single median*

sound radially at all? And what are some of the constant tendencies in this effort?

This problem was taken up in a separate series of tests. Two telephone receivers were placed seven feet apart, eighteen inches above the floor. Another receiver was placed vertically over each of these. All four receivers pointed toward the center of the square thus formed. The observer was seated on a high office stool in such a position that all the receivers were in the median plane of his head, and the center of the square formed by the receivers fell at the center of his head. The sound was produced as before and all the switches were manipulated noiselessly from one position. The quality of the tone was not exactly the same in all the receivers, but the difference was so small that few of the observers could detect it. The intensity was varied systematically between relatively weak and strong sounds. The strong sounds were of the same intensity as in the preceding test, and the other sounds were made so much weaker that the difference was clearly discernible. The test was divided into four parts. In each part two weak and two strong sounds were given through each receiver. The sounds were arranged in such sequence with reference to intensity and direction, that the observer could not guess it or get any help from it. The same order was followed in all the parts and for all the observers.

In the first two parts of the test, the observer remained blindfolded and did not know the number of receivers nor the location of any, except that they were in the median plane. He was placed on a high stool so that he would know that there was ample space for instruments below as well as above. The conditions were the same for the first two parts except that the observer turned around on the stool and faced in the opposite direction in the second part. This was done to eliminate disturbing associations that might accumulate with one position. The last two parts of the test were similar to the first in every respect except that the observer was here shown the location of the receivers and was permitted to keep his eyes open.

This test was made upon twenty-four of the gentlemen who had taken part in the previous test, the results of which had been explained to them. They were blindfolded before entering the room and told that all the sounds would be produced in the median plane, that there would be more sources of sound than in the previous test, that they should not judge by differences in intensity, and that they need only tell which of eight points the sound appeared to be nearest to. These eight points were designated as the four cardinal points, front, down, back and up, and points radially midway between these, counting from the center of the head.

TABLE VI.

Localization of Sounds.

	I		II		III		IV	
	st	*wk*	*st*	*wk*	*st*	*wk*	*st*	*wk*
Up front	37	20	29	26	43	28	43	26
Front	16	8	15	15				
Down front	6	6	4	6	23	22	25	26
Down	2	3	2	2				
Down back	4	10	4	8	9	26	10	23
Back	4	17	11	15				
Up back	15	19	17	17	25	24	22	25
Up	16	17	18	11				

The Roman numerals designate the four parts of the test.
st, strong; *wk*, weak.
The figures in the table give the average per cent of sounds located at the points named. There were twenty-four observers. Each observer had sixty-four trials, distributed equally among the four parts, among the four sources of sound in each part, and between the weak and the strong sounds.

(1.) *Degree of success in localizing the sounds.* In the first two parts of the test, where the observers had no knowledge of the number or the direction of the sources of sound, 12½ per cent of the sounds would be located correctly by chance. These observers get 22 per cent of the strong and 18 per cent of the weak sounds right. The mean residual is 7½ per cent.

In the last two parts of the test, where the observers knew the positions of the four receivers and kept their eyes open, 25 per cent of the sounds would be located correctly by chance.

These observers get 33 per cent of the strong and 33 per cent of the weak sounds right. The residual is 8 per cent.

No observer showed any *special* ability in localizing the sound. There is a remarkable uniformity and the average represents the individual records well.

According to these figures more success is obtained than can be accounted for strictly by chance. But the data are not sufficiently numerous to demand exact conformity to the law of chance. Grant, however, that the residual represents some degree of success and is an approximate measure of it, this small fraction does not correspond at all to the degree of success that most of the observers feel themselves capable of. Most of the observers thought that they had been right very much oftener than this indicates. This does not apply to all, because a few seemed to think that they had to guess all the time. In trying this test myself, I indicated the degree of sureness that I felt by corresponding intensities in the voice when pronouncing the decision. I was about as liable to be wrong when I gave the strongest evidence of sureness as when I gave evidence of uncertainty. The feeling of ability to locate median sounds, that the unpracticed observer has, is almost entirely an illusion. In ordinary experience we have a sort of feeling that somehow we locate sounds that fall in the median plane by direct hearing, while in reality the localization is a matter of inference based upon other experience. Students always show great surprise when the uncertainty of this localization is demonstrated before them in class.

We tried the same test upon a blind man, who walks around in the city and country without any guide. He located no more sounds right than chance requires.

The degree of success is greater when the observers are blindfolded than when they see, practice notwithstanding to the contrary.

(2.) *Constant tendencies.* Some of these may be seen in Table VI. In parts I and II the tendency to misplace the sound upward obtains to about the same extent as in the foregoing test. There is also a tendency, though not so strong,

to misplace the sounds forward. This is like the tendency for all single sounds in the foregoing test.

In ordinary experience we hear more sounds from the region above the horizontal plane through the ears, for the reason that this space is larger and contains more sources of sound than the lower region. We also hear more sounds from objects that we pay attention to, i.e. face, than from objects that we do not attend to, i.e. those behind us. Hence we normally expect more sounds to come from "up front" than from any other direction. Such a tendency ought to reveal itself in the present test. Expectant attention would be focused semi-automatically in this direction, and since there is no clear direct sensation of direction, there should appear a tendency to realize the expectation or to perceive as we habitually perceive. The test was planned to throw some light upon this theory. In Parts III and IV we should expect the constant tendency to decrease because the observers know the positions of the receivers and naturally expect a fair distribution of the sounds.

The results support the theory. The tendency to misplace the sounds in the direction of "up front" is considerably smaller when the positions of the receivers are known than when they are not known. In making a comparison of the figures in Parts I and II with those in Parts III and IV, the average of the weak and the strong sounds must be taken because the weak and the strong sounds followed in the same series without any order apparent to the observer, and inspection of the figures shows that the localization depends upon the differences in intensity to some extent.

In the statement of the degree of success, it has been seen that the success is practically equal for the weak and the strong sounds. That the localization was influenced by the variation in intensity is clearly indicated by the figures which show that there is a tendency to place the weak sounds behind and the strong sounds in front of the observer. The influence of intensity was, therefore, not successfully eliminated and there is nothing to show that the partial success is not due to judgment upon variations in intensity.

IV. Hearing-ability and Discriminative Sensibility for Pitch.

The tests on hearing-ability and the tests on discriminative sensibility for pitch were made upon the same persons and at the same time, and are here reported together for convenience in determining whether any functional relation exists between the two processes.

Hearing-ability of Students.

The original model of the audiometer that is described in a special notice in this volume was employed. (See notice under New Psychological Apparatus.) It was essentially like the one that is described. Slight changes have been made in the standard of intensity and the division of the scale of intensities, therefore no exact comparison can be made between the results obtained with the original model and those obtained with the final model. In this report the intensity of the stimulus is expressed directly in terms of the number of coils in the secondary circuit, that were required to produce the sound. The smaller the number, the more acute is the hearing.

The upper and the lower limits of the threshold of hearing were determined by five trials for each point and the mean of the averages for these is recorded as the threshold of hearing. Both ears were tested in the same way and all the trials were made in the double fatigue order. The stimulus, a double click in the telephone receiver, was always preceded by the usual warning and a sufficient number of control trials were introduced to eliminate the error of fallacious perception. As we had no quiet-room, some of the variations may be due to distracting sounds in the environment. The records for the students that were tested are contained in Table VII.

The first noticeable feature is the great variation in hearing-ability among normal individuals. The difference in the

TABLE VII.

Hearing-ability and Discriminative Sensibility for Pitch.

MEN.

N	*R*	*d*	*L*	*d*	*P*
2	23	1	18	6	12
4	27	1	25	1	12
6	48	7	37	5	12
8	48	11	29	6	12
10	73	3	145	4	30
12	39	6	74	14	30+
14	31	12	43	6	8
16	36	5	68	3	23
18	34	5	21	2	8
20	45	10	50	6	5
22	36	7	50	5	8
24	36	7	56	17	5
26	71	7	37	6	17
28	22	2	40	12	5
30	†				5
32	38	4	28	2	8
34	20	6	22	4	8
36	18	8	16	3	17
38	25	6	18	3	30+
40	†				23
42	56	10	71	4	30+
44	28	6	86	16	3
46	58	4	54	4	12
48	59	13	42	6	3
50	39	8	32	4	8
52	19	6	49	6	5
54	42	10	73	9	5
56	39	9	27	5	30+
58	58	8	38	3	3
Average	39	7	46	5	13

WOMEN.

N	*R*	*d*	*L*	*d*	*P*
1	32	6	27	2	8
3	30	9	20	4	8
5	35	8	32	4	8
7	55	9	58	5	3
9	35	6	24	9	8
11	55	7	74	6	12
13	61	8	55	6	23
15	24	6	43	4	5
17	105	9	*		3
19	18	2	14	3	5
21	26	6	33	7	8
23	43	11	49	17	8
27	48	8	87	8	8
29	82	8	69	12	5
31	24	4	33	3	5
33	50	9	38	6	8
35	19	3	16	4	2
37	15	3	24	6	12
39	17	3	17	3	30+
Average	41	7	40	6	9

N, the observer's number.

R, hearing, right ear; *L*, hearing, left ear; *d*, mean variation.

Unit of measurement for the sound, the energy of one coil in the induced circuit of the audiometer.

P, the least perceptible difference in pitch. (See p. 59.)

Unit of measurement for pitch, 1 v. d.

*Deaf in the left ear.

†Missed test on hearing-ability.

The + signifies that those observers were not tone deaf, but could not perceive differences of 30 v. d.

keenness of the two ears is also conspicuous. Few of the students were aware of the existence of any such difference. The hearing-ability of the men and the women seems to be about equal. These experiments are only preliminary. They have served, at least, to demonstrate the successful operation of a new, convenient, and exact instrument—the audiometer.

Hearing-ability of Children.

The same apparatus and method[1] was employed as with the students; but only from two to four records were made for each limit of the threshold. Sufficient practice was given to

TABLE VIII.

Hearing-ability of Children.

Age	*A*	*B*	*C*	*D*	*E*
8	0	2	6	5	7
9	0	3	5	5	1
10	0	3	7	2	1
11	3	4	10	3	1
12	0	6	10	3	0
13	5	8	5	0	0
14	7	8	4	0	0
15	4	6	3	0	0

The figures give the number of cases that come in each group as specified in the text.

enable the children to understand the nature of the test and recognize the sound. The records of the six and the seven year old children have been discarded on account of the presence of a source of error in the measurement. The results may best be set forth by dividing them into five grades on the basis of keenness in hearing, as follow: Grade A, 1-25; Grade B, 26-50; Grade C, 51-100; Grade D, 101-200; and Grade E, 201-500. The numerals express the intensity of the lowest audible sound as in the foregoing section. The grading is made for the best ear. Table VIII contains the results distributed according to age.

[1] For the general conditions of these tests upon the children, see p. 3.

The records of the boys and the girls are grouped together because there is no noticeable variation with sex. The table shows that hearing-ability improves with age up to the age of 12. Acuteness of hearing depends partly upon general mental ability as shown in ability to comprehend the conditions of the test and concentrate attention upon the particular stimulus, but the test was made so simple and brief that this factor was reduced to a minimum. The variation here shown is therefore probably due chiefly to the development of the sense organ.

The individual differences and the differences between the two ears for the children are as great as for the students. Very few children were aware of the existence of any difference in the keenness of the two ears.

Do bright children hear better than dull children? Before answering this question we must explain the use of the term "general mental ability" in this connection. The children are divided into five grades according to the data furnished by the teachers. The mean per cent of the class standing and the "teachers' estimate of ability" is used as a basis for this division and is here called "general mental ability," for convenience. The designation is open to valid criticism, but may here serve provisionally as a connotation of what is popularly called brightness and dullness.

In Table IX the classification according to general mental ability, as defined, is collated with the classification according to hearing-ability. The same designations, A, B, C, D, and E, are used in both cases. In case of an affirmative answer to the above question, the children of Grade A in general mental ability should come in the higher grades (A or B) for hearing-ability. They do not. The figures in parenthesis show the probable distribution of the cases if there were no connection between hearing-ability and general mental ability. The records follow this distribution pretty closely. Therefore there is here no indication that the bright children hear better than the dull children. There may be cases of children who are dull or are counted dull because they do not hear

well, but such cases are not common enough to be revealed clearly by our method although there may be some indication of them, as in the last figures of Table IX.

TABLE IX.

Comparison of Hearing-ability and General Mental Ability.

Hearing-ability.	*General Mental Ability.* *A*	*B*	*C*	*D*	*E*
A	5 (4)	3 (6)	7 (6)	4 (3)	0 (2)
B	5 (7)	8 (11)	14 (10)	9 (6)	2 (3)
C	11 (9)	18 (13)	12 (13)	3 (7)	1 (3)
D	3 (4)	7 (5)	3 (5)	1 (2)	4 (1)
E	2 (2)	2 (3)	2 (3)	2 (1)	2 (0)

Students' Discriminative Sensibility for Pitch.

(The Measurements were made by Miss Mabel Williams.)

A simple and satisfactory apparatus for this purpose was obtained by tuning a set of forks for increments in height of pitch above international A, 435 v. d., as follows: ½, 1, 2, 3, 5, 8, 12, 17, 23, and 30 v. d. At this point 1 v. d. is equivalent to 1-54 of a tone. The forks are of the best quality and of uniform shape and size—11.5 cm. long. Precautions were taken to keep the temperature constant as that is the greatest source of error. In making the test the standard fork and one of the differential forks were sounded in rapid succession by striking them uniformly and holding them up close to the ear. Each tone was sounded about three seconds, and an interval of about three seconds was allowed between the two tones. The observer was required to state whether the second of the two tones was higher or lower than the first. No other choice of answer was given. The test began with the largest interval, and only one trial was made for each step until the region of uncertainty had been reached. Here ten trials were made for each interval, and the lowest interval for which eight of the ten answers are correct is considered the threshold of discriminative sensibility. This is recorded in the column headed P in Table VII.

The records show the ability of the individual students. The best observer recognizes a difference of two fifty-fourths of a tone, while five are unable to perceive a difference of thirty fifty-fourths of a tone. The superiority of the women's records over the men's may be accounted for by the difference in musical education. There is no marked functional relation between a keen sense of hearing and discriminative sensibility for pitch.

Ability to detect difference in the pitch of tones is a fundamental factor in the appreciation or execution of music. This fact would be better known if all people who pretend to appreciate music had as good power of introspection and were as honest as one of the men tested, who asserted that he appreciated the music of the snare drum as much as any other form of music. The measurement of this ability has not only psychological value but is of pedagogical interest as well. Those who by natural defect of the ear cannot perceive differences between tones cannot appreciate harmony and melody based upon such differences.

There is a peculiar illusion in the discrimination for pitch. Even the smallest intervals appear, to some persons, to be perceptible and recognizable, although they may be below the threshold of discriminative sensibility. The writer experimented with the increment of ½ v. d., having tested the forks with great accuracy. This interval is below his threshold, and yet it appeared to be perceptible in varying degrees. This may be a common illusion among musicians. It seems to be due to the manner of directing the attention in the effort to determine whether the second tone is nearer to a higher or a lower tone that is clearly distinguished from the first and is used as a standard of reference.

Children's Discriminative Sensibility for Pitch.

(This test was made by Miss Della Northey.)

The apparatus and method employed were here essentially the same as those described in the foregoing section. There were two minor differences in the size of the intervals and the

tests were necessarily less complete.[1] The records are divided into five grades according to the magnitude of the least perceptible difference in pitch, which is expressed in terms of the number of vibrations per second, as follows: Grade A, 1-2; Grade B, 3-5; Grade C, 6-10; Grade D, 12-30; and Grade E, those who could not perceive a difference of 30 v. d. For those in Grade E, qualitative tests were made with a chromatic pitch pipe to determine whether there was any case of total tone deafness; no such case was found. Sufficient preliminary

TABLE X.

Children's Discriminative Sensibility for Pitch.

Age	*A*	*B*	*C*	*D*	*E*
6	0	0	6	7	3
7	0	0	6	3	5
8	1	1	9	6	2
9	1	4	0	4	3
10	3	6	3	0	1
11	4	13	4	0	0
12	3	14	3	0	0
13	4	12	4	0	0
14	3	8	7	0	0
15	1	5	6	1	0

The figures give the number of cases that came in each group as specified in the text.

practice was given to teach the child the distinction between higher and lower in pitch. Where this could not be accomplished within the limited time, no record was taken. Still I think that the records for the ages of 6-9 are vitiated by the failure to convey to the child an adequate conception of the nature of the test, within the period of time that was allowed. These records are however included because they show what could be accomplished.

[1]In the tests upon the children the forks were rested upon the table instead of being held up to the ear. We have determined by a special series of tests that, for continuous experiments, this introduces a source of error. The discrimination is much easier if the fork is rested on a sounding board than if it is held in the hand near the ear. This is due to the help that is obtained from differences in timber that are intensified by the sounding board. The tone is purest when the fork is held in the hand.

The record for the boys and the girls are given together in Table X because there is no noticeable variation with sex. For the reason stated, we are not justified in concluding on the basis of these records that there is such development with age as the records for the first four ages may indicate. The reliable records begin with age ten. From ten to fifteen, inclusive, there is no sign of improvement with age in the discriminative sensibility for pitch. The average for all the children for these ages is practically the same as the average for the university women. The answers obtained upon inquiry in regard to the musical training of the children show emphatically that the individual differences are not due principally to training. Many of those who have a high grade of tone sensibility have had no musical education, and the reverse was also found to be true. A child of eight never failed to perceive a difference of two fifty-fourths of a tone. Only a small per cent of adults could reach that limit, even with the

TABLE XI.

Comparison of Hearing ability and Discriminative Sensibility for Pitch.

Discriminative Sensibility for Pitch.	*Hearing-ability.* *A*	*B*	*C*	*D*	*E*
A	2 (3)	4 (6)	12 (7)	1 (2)	0 (1)
B	11 (8)	22 (19)	22 (24)	7 (7)	0 (4)
C	6 (5)	10 (10)	13 (12)	2 (4)	3 (2)
D	0 (2)	3 (4)	3 (5)	3 (2)	4 (1)
E	0 (1)	1 (1)	0 (2)	2 (0)	1 (0)

most persistent practice. The large individual variations, independent of age and sex, must be accounted for chiefly by structural differences in the sense organs.

It is probable that the organ of Corti reaches its maximum efficiency at the age of about ten, and that it then begins to deteriorate, especially if it is not called into systematic activity. This would be analogous to the known fact that the range of perceptible pitch early reaches its maximum extent in children and then gradually narrows down, so that adults do not perceive as high or as low pitch as children.

Do those who have keen sense of hearing tend to have good discriminative sensibility for pitch? The question is answered affirmatively by Table XI in which the data on the two processes are collated. The deviation from the most probable distribution without any functional relation, which is indicated in the parentheses, is in the direction of a correlation between hearing-ability and discriminative sensibility for pitch. But this correlation is not strong.

Do the bright children have a keener discriminative sensibility for pitch than the dull? This is answered by the collating of the classification according to the discriminative sensibility for pitch with the classification according to the general mental ability, in Table XII. There is no functional relation; the distribution of the results practically coincides with the most probable distribution according to chance, which is indicated in the parentheses. This is the strongest

TABLE XII.

Comparison of Discriminative Sensibility for Pitch and General Mental Ability.

Discriminative Sensibility for Pitch.	*General Mental Ability.* *A*	*B*	*C*	*D*	*E*
A	5 (5)	8 (6)	3 (7)	5 (4)	1 (1)
B	16 (14)	18 (16)	18 (17)	7 (10)	2 (3)
C	6 (8)	7 (10)	15 (10)	5 (6)	3 (2)
D	6 (7)	7 (8)	8 (9)	8 (5)	2 (2)
E	4 (3)	3 (3)	3 (4)	2 (2)	1 (1)

evidence in favor of the theory that the discriminative sensibility for pitch depends principally upon the natural structure of the end organ and is subject only to small variation with education.

Miss Mabel Williams has found, in some experiments now in progress in the laboratory, that there is a natural limit to discriminative sensibility for pitch, which may be reached with little or no practice. Some persons experimented upon have been given twenty periods of practice on twenty successive days. Of the three who have had some musical education,

two failed to lower their thresholds at all in that time, and one made some improvement but had a relapse in the trials that were continued after the twentieth day. Of the two who had no musical education, one, a girl aged twelve, could not perceive an interval of 30 v. d. at the beginning, but gradually improved up to a threshold of 5 v. d. in the twentieth trial. The other, a university student, remained at the same threshold, 12 v. d., during the twenty periods of practice.

V. Motor Ability, Reaction-time, Rhythm, and Time Sense.

The following is a statistical study on the time of mental processes.[1] It is an attempt to bring together some of the approved experiments and reduce them to a reliable statistical form. The measurements are made upon a well defined group of persons, under uniform conditions, and with reliable apparatus and methods. The student observers are designated by the same numbers in all the tables of results.

1. Time of Action, Simple Reaction, Discrimination, and Choice.

The following written directions to the observer indicate the nature of the tests in this series:

"There are five sets of experiments in this series and all should be made under the same general conditions. The warning 'Now' will be given from two to five seconds before each stimulus. React as quickly as possible and do not allow yourself to be disturbed by any form of distraction. In Sets A, B, and C, do not direct your attention toward the stimu-

[1]It is perhaps unnecessary to state that the expression "time sense" does not imply that we possess a special sense of time. And the separation of the processes in complex reaction-time, does not imply that any abrupt psychical divisions exist.

lus, but concentrate all thought and effort upon the movement to be made by the finger.

A. Simple reaction to touch. Press the button as soon as you are touched on the forehead.

B. Simple reaction to sound. Press the button as soon as you hear the click in the receiver.

C. Simple reaction to light. A light will appear; press the button as soon as you see it.

D. Reaction after discrimination in sight. Either one or two lights will appear; press the button as soon as you have distinguished whether there is one or two, and immediately call out the appropriate number.[1]

E. Reaction after discrimination and choice. Either one or two lights may appear; if one, press the button; if two, do not.

F. (Special) Greatest rapidity and regularity of action. Press the key, with a small movement of the index finger of the right hand, as rapidly and regularly as possible."

All the records were taken by the graphic method. A Depréz marker was so connected that it recorded the application of the stimulus and the following reaction. The records were read on a 100 v. d. time-line. Although the records were made in such a way and under such conditions that they could have been read in thousandths of a second, they are given only in hundredths of a second. The reaction was made with a break key, except in Set A where it was necessary to use a make key. The excursion of the key was reduced to a minimum. Latent times have been eliminated. Special reference must be made to the stimuli and the conditions of each test.

A. The Scripture touch-key[2] was used by an assistant in touching the forehead of the observer. A light tap with the hard rubber point of this produces a distinct sensation of impact. The missing records in the accompanying tables were thrown out because in obtaining them the make-contact of

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[1] This is a satisfactory control method. The observer will call out the number for which he reacted whether the reaction is right or wrong.

[2] See Stud. Yale Psych. Lab., 1895, III. 107.

this key was used, and that introduced a too great source of error. With the light break, that was used afterward, the record was made upon the beginning of the impact.

B. A telephone click served as sound stimulus. Its intensity was such as to make it clear and yet not disagreeable.

C. The sight stimulus consisted of a light produced by the fluorescence of a Crooke's tube, seen through an aperture, 10 mm. in diameter, at the back of a dark-cabinet, 1 M deep. The tube was illuminated by one hundred flashes per second. This is a more accurate sight stimulus than can be produced by any form of a shutter, and obviates the sound accompanying the spark.

D. Here the same stimulus was used as in Set C, with the only variation that a second aperture could be left open at the control of the experimenter, thus producing two similar lights. The two apertures were 20 mm. apart. The adjustment for one or for two lights could be made noiselessly between the trials. Eight single and seven double lights were used.

E. The stimulus was the same as in Set D. Ten double lights were interspersed in irregular order with fifteen single lights.

F. The button of the key made an excursion of about 3 mm. and required a pressure of nearly 100 g. The record was taken at the end of seven seconds of tapping. The observer did not know that the record for the first seven seconds was not taken. The period recorded includes a complete movement of the finger.

About eight trials were given for practice in each of the reaction tests. The trials were made in the double fatigue order for the series of tests. No clear distinction can be made between sensory and motor reactions for untrained observers, but the directions aimed to make the simple reactions of the motor type. All the conditions were reduced to such as were simplest and most favorable for quick and accurate response. The results are contained in Table XIII.

The relative length of the simple reaction-times varies in

the generally accepted order. Reaction to hearing is the shortest and most uniform, reaction to touch upon the forehead is next in order of length, and reaction to sight requires the longest time.

The discrimination-time is a more definite quantity than it has generally been considered to be. The average time for the men is 0.08 sec., and for the women 0.07 sec. The mean variation is no greater for the gross discrimination-time than for the gross time which is terminated by choice.

The simple choice-time is a trifle longer than the simple discrimination-time. It is 0.10 sec. for the men and 0.08 sec. for the women.

Erroneous reactions may operate in one of two ways: some persons shorten the average complex reaction-time by taking the chance of incurring some errors, and others are hampered by the occurrence of an error so that they become over-cautious and lose time in hesitation. Thus premature reactions may signify a gain to some and a loss to others.

The fact that a proportionally smaller number of errors are made in Set D than in Set E indicates that the premature choice-reactions are not due to a failure to discriminate before acting, but to a failure to inhibit the habitual action.

While in general there seems to be a fairly constant ratio between the different simple reaction-times, and between the simple and the compound reaction-times, there are some conspicuous exceptions that point to individual peculiarities in the attitude toward the different senses and peculiarities in the relative readiness of thought and action. Thus there are indications of ear-mindedness and of eye-mindedness, so also of the deliberate and accurate observer and of the erratic and impulsive actor. These signs are most interesting to one who is acquainted with the observers.

The complete time for the most rapidly and regularly repeated movement (F) is equal to the shortest reaction-time (B); i. e. the double motor process in Set F requires the same length of time as the combined sensory and motor processes in Set B. It may be that the repeated movements would have

TABLE XIII. (*A.*) *Men.*

Time of Action, Reaction, Discrimination, and Choice.

N	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*e*	*E*	*d*	*f*	*F*	*d*
2			13	2	21	2	35	9	0	38	7	0	14	1
4			16	2	20	2	29	3	1	39	7	2	18	1
6			15	3	22	8	26	3	0	32	9	0	16	1
8			13	2	20	2	31	7	0	34	3	1	12	0
10			17	2	19	3	27	4	0	51	7	2	17	1
12			13	4	23	2	25	4	0	27	7	2	18	1
14			13	2	17	2	25	4	2	32	5	1	14	1
16	21	7	16	3	21	3	25	3	0	33	4	2	12	0
18	18	1	15	2	22	1	28	3	1	41	7	1	17	1
20	15	1	12	1	18	2	24	4	0	38	5	1	13	0
22	20	3	17	4	21	4	25	6	1	51	14	1	14	1
24	18	3	14	3	25	4	37	8	0	45	8	2	15	1
26	19	2	17	4	22	3	23	3	0	32	5	3	12	1
28			14	2	22	3	31	5	0	46	8	2	17	1
30	19	2	14	1	23	5	26	4	0	43	6	1	12	1
32	19	2	16	4	20	4	24	5	0	39	9	2	16	1
34	23	3	15	2	23	4	47	10	0	46	9	0	15	1
36	24	2	15	1	19	2	28	5	0	37	4	1	15	1
38	17	2	13	1	21	3	24	3	0	39	6	0	13	1
40	17	2	13	1	20	3	27	3	0	30	4	1	12	2
42	15	2	13	1	22	1	32	8	1	39	5	2	14	0
44	17	2	11	1	15	2	23	4	1	28	5	2	14	1
46	15	2	12	1	19	3	33	6	1	44	4	1	14	1
48	16	3	12	1	21	2	41	1	0	60	6	0	15	1
50	17	1	14	1	23	3	29	6	0	33	2	1	16	1
52	17	1	13	2	20	4	44	6	0	42	5	1	16	3
54	21	2	16	3	23	2	31	8	0	36	4	3	14	1
56	19	4	15	3	19	2	24	4	0	34	4	2	18	1
58	16	3	13	1	20	5	28	7	0	38	5	0	11	1
Average	18	2	14	3	21	3	29	5		39	6		15	1

Unit of measurement, 0.01 sec.

A, B, C, D, E, and F, the tests as designated in the "directions."

d, mean variation for the fifteen trials on each point. This is the average of each individual's variations, regardless of sign, from the average record for all the observers.

e, number of premature reactions. This represents half the probable number because half of the premature reactions would be right by chance.

f, number of errors made (in ten control trials) by premature reactions, i. e. by reacting to two lights.

TABLE XIII. Continued. (*B.*) *Women.*

N	*A*	*d*	*B*	*d*	*C*	*d*	*D*	*d*	*e*	*E*	*d*	*f*	*F*	*d*
1			21	4	31	3	35	6	0	42	3	0	17	1
3			14	2	19	4	38	11	0	44	13	1	13	1
5			15	2	18	2	28	3	0	34	5	2	13	0
7			13	1	24	3	36	5	0	44	4	1	13	0
9			16	1	23	2	39	6	0	45	5	1	20	1
11			26	4	27	7	33	7	0	39	4	1	22	1
13	17	3	12	1	22	2	30	4	0	39	4	3	13	1
15	17	2	12	2	22	4	30	5	0	37	6	1	12	2
17	16	3	14	1	16	3	23	5	0	45	5	1	15	1
19	25	8	15	2	22	2	26	3	0	39	4	1	14	0
21	19	2	15	1	22	2	29	6	0	32	4	0	18	2
23	18	2	14	2	20	7	31	4	1	36	4	2	14	1
25	19	3	18	2	22	3	47	10	0	45	10	1	15	0
27	13	4	13	2	16	4	22	2	0	37	5	2	15	1
29	23	5	19	1	25	4	32	7	0	43	11	2	16	0
31	17	1	15	3	17	3	23	4	1	40	8	0	16	1
33	16	1	12	1	18	2	23	5	1	29	6	3	12	2
35	21	2	13	2	26	2	33	4	0	41	5	3	12	0
37	22	2	15	1	26	3	27	3	0	34	5	2	14	0
39	14	3	14	2	21	3	31	11	0	37	4	0	13	1
41	18	2	18	2	22	3	36	11	0	43	3	1	16	1
43	15	3	13	2	25	4	29	6	0	42	9	2	16	1
45	18	2	13	2	23	2	27	5	1	31	5	2	11	1
47	15	1	13	2	20	3	28	3	0	38	8	1	12	1
49	18	3	14	2	19	1	29	5	0	30	2	1	16	1
51	15	1	13	1	20	3	27	5	0	34	4	2	14	0
53	16	2	15	2	24	5	29	3	0	39	9	0	14	1
Average	18	3	15	2	23	3	30	6		38	6		15	1

been shorter in some cases if the regularity had not been demanded, but it is not probable that many sacrificed speed for regularity. There is a tendency for those who have good voluntary motor ability to have short reaction-time, both simple and complex.

There is no remarkable variation with sex in time, uniformity, or reliability. Although it is not necessary to do so, the small variations that do exist may be accounted for by the normal fluctuations in the records.

2. *Free Rhythm in Action.*

The purpose of this test was to determine the most natural rhythm of action and its characteristics in free, simple, and

small movements of a limb in its most natural position. It was required to repeat a light pressure rhythmically by the tip of the first finger.

The Verdin capsule[1] for the study of small movements consists of a metal lever, 20 mm. long and 2 mm. in diameter, mounted vertically on the membrane of an ordinary capsule for the transmission of air pressure. This capsule was connected with a Marey recording air capsule which was mounted on a kymograph. By this means a tracing was obtained showing the form, amount, and duration of the successive pressures that were exerted upon the end of the lever of the Verdin capsule by the observer's voluntary action.

The word rhythm was not mentioned and precautions were taken to prevent the suggestion of any particular rate. The observer was required to press with a force of about 8 g., which required an excursion of about ·5 mm. by the finger. The standard was indicated to the observer in some single preliminary trials. During the test the eyes were kept closed. The specific instructions were given in writing, as follows:

"Rest your hand on the table and press this point with the first finger, at regular intervals, always in the same way. Choose any length of interval you like, but retain the same throughout the experiment. The regularity of the interval and the uniformity of the pressure will be recorded."

As the records do not manifest any peculiarity in the mode of pressing, that aspect is disregarded and the results are stated with reference to the length of the periods and the degree of pressure. The former is recorded in columns *A*, *B*, and *C* and the latter in columns *D*, *E*, and *F* in Table XIV. The length of the periods of the rhythm was determined at the beginning, the middle, and the end of each test, by measuring, with a time-line, the distance from crest to crest in five successive waves. The time-line scale was divided into twentieths of a second. The records of the pressure have not been converted into units of weight, but are retained in terms of the original measurement of the amplitudes of the graphic

[1] See Catalogue of Ch. Verdin, Paris.

TABLE XIV.

Free Rhythm in Action.

(*A.*) *Men.*

	Period			*Pressure*		
N	*A*	*B*	*C*	*D*	*E*	*F*
2	18	18	18	11	14	14
4	18	16	15	10	22	27
6	11	11	11	14	24	29
8	22	22	22	7	22	31
10	12	11	10	14	32	34
12	28	28	28	6	6	6
14	26	26	26	9	16	24
16	25	25	25	7	18	43
18	14	13	12	10	13	19
20	17	17	17	4	5	9
22	13	13	13	11	14	20
24	34	34	34	6	22	29
26	28	28	28	10	17	21
28	15	15	14	7	6	10
30	14	13	12	7	11	22
32	24	26	28	8	14	19
36	28	28	28	7	15	20
38	12	12	12	4	5	4
40	11	11	11	6	8	15
42	25	25	25	2	4	5
44	13	13	13	7	8	15
46	6	6	6	12	23	47
48	13	13	13	7	8	15
50	30	30	30	9	23	22
52	17	16	14	6	10	13
54	14	16	26	2	3	6
56	16	16	16	6	9	18
58	14	14	14	6	17	30
			Average	8	14	21

(*B.*) *Women.*

	Period			*Pressure*		
N	*A*	*B*	*C*	*D*	*E*	*F*
1	15	15	15	5	6	6
3	8	8	8	4	15	16
5	11	11	11	6	18	26
9	15	15	15	14	20	22
11	8	10	10	5	6	7
13	10	10	10	14	17	34
15	12	12	12	7	10	13
17	16	15	14	9	32	38
19	18	16	15	8	14	17
21	12	12	12	10	42	51
23	70	70	70	11	30	41
25	40	40	40	4	15	16
27	18	20	24	16	14	11
29	60	60	60	10	19	25
31	10	13	14	8	13	18
33	36	44	48	12	18	38
35	12	12	12	5	5	5
37	9	9	9	8	10	17
41	22	22	22	8	12	16
43	23	23	23	8	8	8
45	20	21	24	8	16	18
49	9	8	7	7	8	9
51	10	10	10	9	13	16
53	8	8	8	5	6	7
55	32	32	35	8	6	6
57	13	13	13	9	14	22
59	10	12	14	8	16	18
			Average	8	15	19

N, the observers by number.

Length of the intervals in the chosen rhythms: *A*, at the beginning; *B*, at the end of 45 sec.; and *C*, at the end of 90 sec. The figures give twentieths of a second which was the unit of measurement.

Pressure: *D*, at the beginning; *E*, at the end of 45 sec.; and *F*, at the end of 90 sec. The figures give the amplitude of the graphic waves in millimeters.

waves, because the conversion would not be reliable and the record here given is expressive enough for the present purposes.[1]

The rhythm of these free movements seems as a rule to be determined by the periodicity of the processes of circulation and respiration. The most frequent rhythm is that of the pulse. There may be three cases. (1) The period of the complete movement may be synchronous with the period of the pulse beat. In conditions like those under which this test was made, the pulse may vary, at least, from sixty-six to ninety-two beats per minute in different persons. That is equivalent to pulsations of from 18 to 13 twentieths of a second in length. Nineteen of the observers chose rhythms that fall within these limits. (2) The period of action may be a multiple of the pulse beat. The downward and the upward movements of the finger may be considered as separate movements and each made to coincide with a pulse beat. According to the above estimate of possible pulse rates, periods of action of from 36 to 26 twentieths of a second would correspond to double periods of the pulse. Eight of the records fall within these limits. (3) Two complete movements may coincide with one beat of the pulse. Five of the records fall within such limits, i. e. 8 to 7 twentieths of a second. Observers 23, 25, 29, and 33, were seen to follow the

[1] The recording of pressures by air transmission is not satisfactory, as the functional relation between the pressure and the amplitude of the curve is too uncertain. The recording may be made directly with a very simple device. Take a light wooden lever and support one end of it on pivot bearings and the other from a suspended coil spring. Let a spring pointer project from the side of the lever and trace on a kymograph drum, and suspend a pressure button below the lever or in any other direction by means of pulleys. The amplitude of the graphic record may be varied by placing the tracing point at different distances from the bearing of the lever. The force of the pressure and the amplitude of the movement may be adjusted by changing the point of suspension of the pressure button in a similar manner. Large variations in pressure must be adjusted for by substitution of springs. The friction on the drum will be negligible for most purposes if the tracing point is long and flexible laterally. This dynamograph makes an excellent ergograph. If a constant pressure is desired, a weight acting over a pulley may be substituted for the spring.

rhythm of the respiratory movement. In many other cases it was evident that the respiratory periods were regulated so as to form some multiple of the periods of action. In that way the rhythm of action may have been correlated with both the circulation and the respiration. None of the observers were aware of having followed the pulse or the respiration.

Is the rhythm of action correlated with the pulse at the moment of action or with the average normal pulse? That is, is the subconscious correlation of the two processes direct, or is the chosen rhythm that of the habitual movements whose rhythmic character has been gradually determined by the subconscious influences? That important question is merely suggested by these experiments. The problem is worth studying quantitatively by means of synchronous records of the action, the pulse, and the respiration. But this cannot be done by means of the ordinary sphygmograph and pneumograph, because these instruments suggest special rhythms and, by pressure, bring the pulse and the respiration into the focus of attention. It is necessary that the record of the organic processes be made without the knowledge of the observer.

The regularity with which the chosen rhythm is adhered to is remarkable. If a person be required to act with a given rhythm, e. g. two second intervals, there will appear a strong tendency to acceleration. (See next section.) The absence of such a tendency here may indicate that the regularity of the rhythm was determined by some present gauge like the processes that have been mentioned.

There is a strong and constant tendency to increase the degree of pressure during the free, rhythmic activity. As may be seen in columns *D*, *E*, and *F* in Table XIV, there are only four exceptions to this rule in the fifty-five cases. The amount of increase is remarkable. The figures in the tables that record the amplitude of the waves in the graphic tracings do not fully represent the amount of increase because the deflection of the tracing pointer is relatively less for the greater pressures. The average pressure is at least three times as great at the end of the ninety seconds as at the

beginning. No reference was made to the standard of pressure after it had been once indicated in the preliminary trials. This accounts for the difference in standard that is chosen in the beginning by the various observers. All were astonished at the amount of increase shown by the graphic records.

There is a slight tendency for the women to choose faster rhythms than the men. Thus eight of the women chose periods of 10 twentieths of a second, or less, while only one of the men chose a rate as fast as that. On the other hand the women also chose the longest periods. Five women and only two men chose periods of 30 twentieths of a second, or more. This is in accord with the frequent observation that the actions of a given class of men are more uniform than the actions of a corresponding class of women. This difference appears in a similar way in other experiments, as may be seen from the tables.

3. Regulated Rhythm in Action.

The purpose of this test was to obtain a measurement of the ability to reproduce or follow a fixed rhythm, and to determine some of the constant tendencies in such action.

The latest model of the Meumann time sense apparatus was used on a Zimmermann kymograph. The rhythm was marked by a sounder connected with this apparatus. The records were traced by a Depréz marker in circuit with a telegraph key with which the observer indicated the rhythm. All latent times have been eliminated.

Three rhythms were chosen to represent respectively the slowest, the most favorable, and the fastest movements that could be performed by all the observers within the limits of accuracy to be specified. A period of 2.80 sec. was adopted as the longest that all could reproduce with less error than plus or minus the reaction-time; a period of 0.48 sec. was taken as the shortest that all could reproduce without confusion; and a period of 1.08 sec. was selected as perhaps the most favorable. These three rhythms constituted one series. Sufficient practice was given in which to become familiar

with the action of the apparatus and acquire the rhythmic movement. Then, without stopping, forty records were made in succession with the same standard. The same procedure was followed for each rhythm, the tests being made in the order the records are given in the tables. During the test the observers kept their eyes closed. The written instructions were given as follows:

"Hold the key like a telegrapher, observe the regular beat of the sounder, and mark this time by pressing the key so that the click of the closing of the key coincides with the click of the sounder."

Table XV sets forth the individual and the general characteristics that are exhibited in these forms of regulated rhythmic action. It gives a sort of birds-eye view of the class. The records show tendencies to anticipate, to lag, to fluctuate, and to be exact; and these tendencies are indices to the temperament, and the habitual modes of perception, attention, and action.

In comparing the degrees of success for the different rates, a standard is chosen approximately proportional to each period. Thus, the trial is considered a success if the period is estimated correctly within ± 0.05 sec. in rate I, ± 0.02 sec. in rate II, and ± 0.01 sec. in rate III. Considering both the standard of success and the number of successful estimates, we find that the ratio of success is about the same for all three rates, i. e. the degree of success is proportional to the length of the period.

There is a strong tendency to underestimate the period, i. e. to anticipate the regulating click in the fast and the medium rates. This is due to the tendency to accelerate a given rhythm. With many of the observers there was a noticeable rhythm in this acceleration itself. Some observers would gradually accelerate until it dawned upon them that they were constantly anticipating; they would then make a fresh start and soon find that they were again accelerating. Others plainly made a uniform effort to check the tendency to accelerate in the latter part of the test. Several protested

TABLE XV. (*A.*) *Men.*

Regulated Rhythm in Action.

	I. Slow: 2.80 sec.					*II. Medium: 1.08 sec.*					*III. Fast: 0.48 sec.*				
N	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*
2	32	60	21	8	9	8	92	7	0		30	45	3	5	3
4	0	100	32	0		36	49	7	15	7	34	19	2	47	3
6	41	46	16	13	10	35	65	9	0		37	41	2	22	2
8	15	23	22	62	18	38	25	4	37	7	66	11	3	23	3
10	39	36	13	25	15	20	72	7	8	5	5	95	5	0	
12	23	49	14	28	11	13	74	16	13	13	18	72	6	10	3
14	15	60	23	25	13	20	67	8	13	5	22	66	4	12	3
16	24	52	13	14	7	8	92	9	0		24	66	5	10	3
18	18	32	17	50	10	31	56	7	13	4	14	76	5	10	3
20	28	59	17	13	9	19	43	6	38	5	30	66	8	4	2
22	18	46	22	36	15	23	72	9	5	3	23	64	3	13	3
24	35	23	37	42	28	15	80	8	5	4	23	64	3	13	2
26	21	64	14	15	11	33	67	6	0						
28	22	53	29	25	12	31	36	7	33	7	18	46	8	36	8
30	0	0		100	17	23	0		77	7	13	5	2	82	3
32	17	35	17	48	12	26	59	11	15	11	10	90	6	0	
36	21	37	14	22	15	47	27	5	26	5	47	38	3	15	2
38	42	11	9	47	13	45	22	5	33	5	42	51	3	7	3
40	39	35	14	26	11	40	38	5	22	5	19	81	4	0	
42	0	0		100	31	5	8	5	87	13	29	23	3	48	4
44	24	38	14	38	9	35	44	5	21	6	64	12	3	24	2
46	33	59	24	8	10	18	68	6	14	5	27	68	3	5	3
48	21	44	22	35	16	21	57	5	23	4	47	27	3	26	3
50	15	60	26	25	17	38	62	6	0		39	53	3	8	3
52	5	8	15	87	23	27	73	8	0		50	33	3	17	2
54	0	0		100	14	20	5	5	75	10	11	89	4	0	
56	20	30	18	50	22	46	30	3	24	5	59	8	2	33	2
58	29	55	20	16	10	21	74	8	5	5	38	62	3	0	
62	23	63	12	14	9	13	87	10	0		5	95	6	0	
64	28	22	28	50	25	23	71	5	6	5	2	98	6	0	
66	26	50	19	24	14	0	100	18	0		0	100	6	0	
Av.	22	40	19	38	15	26	55	7	19	6	29	55	4	16	3

Unit of measurement, 0.01 sec.

Number of trials, 40 for each rate.

N, the observers by number.

%*R*, per cent of cases right within ± 0.03 sec. in rate I; within ± 0.02 sec. in rate II; and within ± 0.01 sec. in rate III.

%*U*, per cent of cases in which the observer underestimated the period.

%*O*, per cent of cases in which the observer overestimated the period.

AU, average time of underestimation, i. e. anticipation.

AO, average time of overestimation.

TABLE XV. Continued. (*B.*) *Women.*

	I. Slow: 2.80 sec.					*II. Medium: 1.08 sec.*					*III. Fast: 0.48 sec.*				
N	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*	%*R*	%*U*	*AU*	%*O*	*AO*
1	32	34	20	34	10	0	100	28	0		0	100	7	0	
3	33	8	12	59	12	33	27	6	40	8	33	41	3	26	3
5	4	50	20	46	12	0	100	14	0		0	100	14	0	
9	13	50	19	37	16	20	36	7	44	6	3	97	8	0	
11	27	24	14	69	12	2	0		98	9	10	71	5	19	4
13	25	16	16	59	16	12	82	10	6	4	20	67	4	13	4
15	25	44	15	31	12	19	66	6	15	7	23	70	5	7	6
17	3	3	7	94	16	42	44	7	14	5	8	80	6	12	3
19	15	10	15	50	29	0	100	18	0		5	95	5	0	
21	5	25	27	70	12	10	87	11	3	8	11	78	3	11	3
23	23	8	10	69	13	9	60	9	31	8	59	27	4	14	3
25	28	37	19	40	21	19	56	6	25	6	53	31	3	16	2
27	6	14	11	80	13	31	9	4	60	7	38	15	2	47	2
29	11	31	20	58	14	33	35	8	32	7	25	50	3	25	3
31	11	8	23	7	6	13	82	9	5	5	0	100	5	0	
33	14	67	53	19	18	29	53	12	18	7	27	66	3	12	3
35	20	35	20	45	20	38	26	5	38	4	43	57	3	0	
41	13	53	62	34	21	5	92	12	3	3	15	82	4	3	3
43	20	22	18	58	23	28	63	6	9	6	51	35	2	14	2
45	10	70	32	20	15	38	51	6	11	4	20	80	4	0	
49	15	10	27	75	30	17	83	8	0		25	68	4	7	3
51	52	37	13	11	9	50	36	5	14	6	21	68	6	11	4
53	17	83	21	0		7	93	8	0		25	48	4	27	3
55	0	5	24	95	27	20	74	9	6	5	32	42	3	26	3
57	18	45	21	37	22	44	40	5	16	4	20	80	4	0	
59	23	20	17	57	17	58	30	5	12	4	46	37	3	17	3
Aver.	18	31	21	51	17	22	59	9	19	6	24	64	5	12	3

that the standard rhythm had gradually become slower; they considered their action uniform and ascribed the variation to the regulating apparatus.

The men have somewhat better records than the women in all three rhythms, both in regard to the per cent of successful trials and in the degree of approximation to the standard.

4. *Time-estimate.*

The following is an attempt to determine some of the constant tendencies in the appreciation of intervals of time. The investigation is limited to one kind of interval, namely, the

so-called "empty" interval. The following lengths of interval were chosen: ¼ sec., ½ sec., 1 sec., 2 sec., 5 sec., 10 sec., 20 sec., and 40 sec. These intervals were divided into two series, the first four being in one and the last four in the other.

First Series: Short Intervals.

The Meumann time sense apparatus was used to produce clicks in a sounder. By this apparatus time intervals of the lengths here required could be marked off with uniformity and precision. The latent time of the sounder was eliminated. The records were made on the kymograph by the usual graphic

TABLE XVI. (*A.*) *Men.*

Time-estimate: Short Intervals.

N	*25*	*d*	*50*	*d*	*100*	*d*	*200*	*d*
6	24	2	55	7	78	7	179	18
12	27	4	51	6	87	7	181	18
16	24	5	47	4	83	15	152	22
18	25	4	49	5	88	7	181	17
22	26	6	47	8	89	10	164	11
24	25	2	48	3	84	4	129	22
28	47	17	56	9	88	17	127	8
30	25	2	48	4	95	4	181	9
32	33	2	63	9	91	16	147	8
36	29	2	64	7	110	11	197	20
38	28	2	54	6	85	10	132	9
40	26	3	46	2	84	15	129	12
42	44	5	72	13	101	18	173	19
46	23	2	51	9	96	13	117	7
48	23	2	48	4	88	11	138	12
52	32	10	39	4	70	13	166	19
54	39	10	54	6	94	6	141	12
56	33	7	59	6	100	14	189	18
58	28	6	55	6	82	15	185	19
60	20	3	46		94		169	16
Average	29	5	53	6	89	12	159	14

The numbers at the head of the columns give the standard intervals.
Unit of measurement, 0.01 sec.
N, the observers by number.
d, mean variation.

TABLE XVI. Continued. (*B.*) *Women.*

N	*25*	*d*	*50*	*d*	*100*	*d*	*200*	*d*
1	27	6	57	11	85	11	160	25
3	28	4	50	9	95	11	182	11
5	30	3	40	8	99	0	136	10
7	26	8	55	6	93	7	154	14
9	31	4	52	5	85	6	174	31
13	24	4	53	5	94	3	169	21
15	27	2	44	5	98	4	175	6
17	23	3	56	5	103	9	197	15
19	25	2	47	5	97	1	159	19
21	34	6	50	10	76	10	151	15
23	34	3	60	4	98	5	141	13
25	32	6	61	6	96	12	138	6
27	39	12	68	11	91	14	192	7
29	30		49	2	73		183	18
31	32	7	55	9	94	7	182	29
33	27	2	46	5	82	12	113	18
35	38	3	48	3	93	9	172	12
37	32	3	52	3	92	5	121	10
41	38	4	66	9	105	5	152	8
43	22	2	48	4	91	4	129	4
45	27	3	37	3	78	15	108	9
49	31	8	41	4	91	10	134	14
51	27	2	54	8	91	7	184	16
55	28	3	57	8	92	11	129	11
57	26	3	47	8	97	13	180	20
59	38	2	52	13	95	14	179	16
Average	29	4	52	6	92	9	157	15

method and were read in hundredths of a second. Eight trials were made for each interval in the double fatigue order. The observers did not know anything about the length of the intervals or their order in the series, except what could be learned in the progress of the actual test. Special instructions were given in writing as follows:

"The experimenter marks off a certain interval of time by two clicks. After about two seconds, reproduce it by two similar clicks with the key. *Cautions:* Try to think of the passing time only. Do not count, estimate number of seconds, or use any mechanical aid. If any such process occurs invol-

untarily the trial must be repeated. In pressing the key act with precision."

The short intervals are overestimated and the long intervals are underestimated by amounts shown in Table XII. This tendency is uniform and the uniformity is in no sense due to the observers' knowledge about each others records. Men and women show equal ability.

A comparison of Tables XIII and XVI shows that the observers, who overestimate the shortest interval most, tend to have poor motor ability and reaction-time. But the overestimation of this interval is not due to inability to act quickly enough, because two taps can be made in as rapid succession as a series of taps can be made, and no one required more than 0.20 sec.

TABLE XVII. (*A.*) *Men.*

Time-estimate: Long Intervals.

N	*5*	*d*	*10*	*d*	*20*	*d*	*40*	*d*
6	5	1	10	1	19	2	33	4
12	5	1	10	3	16	2	31	4
16	5	1	9	3	20	2	36	3
18	5	1	10	1	18	3	35	4
22	5	1	10	1	20	3	33	4
24	7	2	10	2	22	5	40	2
28	4	2	7	3	13	2	29	5
30	6	2	11	3	15	2	26	3
32	7	1	11	2	25	6	33	6
36	5	1	13	4	20	3	42	10
38	4	1	8	1	11	1	17	1
40	6	2	12	4	24	6	45	10
42	9	3	11	4	25	5	33	4
46	5	2	11	3	18	5	34	8
48	5	2	9	5	23	10	40	6
52	5	1	9	1	17	2	36	3
54	5	1	12	2	19	1	35	2
56	6	2	12	4	22	4	35	10
58	5	1	10	2	22	3	36	7
60	5	2	9	2	16	4	44	7
Aver.	5.5	2	10.2	3	19.3	4	34.7	5

Unit of measurement, 1 sec.
Other notation same as in Table XVI.

TABLE XVII. Continued. (*B.*) *Women.*

N	*5*	*d*	*10*	*d*	*20*	*d*	*40*	*d*
1	4	1	7	1	15	3	27	5
3	3	1	9	1	23	0	34	2
5	4	2	6	1	22	8	31	2
7	3	0	10	4	13	3	34	15
9	5	1	9	2	22	1	35	7
13	5	1	10	2	17	6	28	5
15	6	1	13	5	20	4	33	3
17	4	2	6	1	13	3	27	4
19	6	2	10	1	22	4	44	8
21	4	1	7	2	12	4	23	2
23	5	1	8	1	12	2	29	2
25	6	1	11	3	17	3	44	4
27	5	1	10	2	20	1	51	6
29	9	2	14	4	35	3	46	7
31	10	3	18	4	31	9	34	5
35	6	2	10	5	15	3	28	2
37	4	1	9	2	15	3	26	2
41	4	0	9	0	16	4	39	7
43	4	0	7	1	14	2	35	10
45	4	1	9	1	21	3	41	4
49	4	1	6	1	11	2	26	5
55	5	1	9	2	18	1	39	4
57	3	1	9	2	19	4	36	4
59	4	1	8	1	17	1	33	7
Aver.	4.9	1	8.9	2	18.3	4	34.3	5

(F in Tab. XIII) for the complete movement in tapping, which was exactly the same as the movement in the signalling in this test. Fearing that the overestimation might still be due to the inability to act, I called back some of those who had overestimated, repeated the trial, and asked them whether they were aware of the fact that they had overestimated. No one was distinctly aware of the overestimation and no one thought that the interval was too short to be reproduced.

It is to be regretted that I cannot give a more adequate account of the observations upon this very important point. However, the gauging of the smallest interval by the measured voluntary motor ability, and the information elicited through questions in the extra trials, convince me that this overestima-

tion of the short interval is not due to inability to act quickly enough, but it is a normal illusion in the perception of the standard interval. The evidences warrant the conclusion that the time that is associated with his own quick actions seems shorter to the slow person than to the quick person. Does the slow boy realize how long an interval elapses before he begins to reply to the teacher's question? And, when he has once started, do the intervals between his words seem as long to him as they seem to the quick boy at his side? The lagging of a slow person in all rushing activities does not seem as great to him as it does to the quick person. Persons who are habitually too slow in their quickest actions have established erroneous associations between standards of time and the time of their own actions.

Second Series: Long Intervals.

(This test was made by Miss Anna Kierulff.)

This test followed immediately upon the foregoing and was made as nearly as possible under the same conditions. The experimenter kept time with a Runné chronometer and signalled with the sounder by pressing an electric key. The observer had a similar key and signalled with the same sounder at the beginning and at the end of the interval. The chronometer was equipped with an electric starting and stopping attachment in such a way that the interruption of the circuit that produced the first signal started the chronometer and the interruption that produced the second signal stopped it. The observer was seated at a distance of forty feet from the experimenter, and kept the eyes closed. Four records were taken upon each point in the double fatigue order. The written instructions that had been given before were repeated, and a few preliminary trials were given with intervals chosen at random within the limits of from five to fifteen seconds. The results are contained in Table XVII.

In this series the shortest interval is estimated almost correctly, and the other intervals are underestimated somewhat in proportion to their length. But the shortest interval in this

series is two and a half times as long as the longest interval in the foregoing series, which is underestimated by nearly one-fourth of the actual length. The conclusion is evident, that the tendencies to overestimate and to underestimate are not absolute. They are related to the setting, as it were, in the associated intervals. The two parts of this experiment became, to the observer, two different groups of time relations with little or no effect upon each other. If they had been reduced to one unbroken series, tried with the same apparatus, and with the observer in the same position, it is probable that the illusory effects would have been stronger for the extremes, ¼ sec. and 40 sec., and that the tendency to underestimate would not have appeared for the four short intervals. The variation that depends upon the grouping of the intervals is due to the bodily and the mental attitudes of expectancy that are created by the grouping.

Third Series: Variation with Mental Development.

(These tests were made by Miss Mary Hornbrook.)

The Second Series of tests were repeated upon school children. (See description of the general method and conditions, p. 3.) Sight signals were substituted for the sound signals in order to simplify the conditions. The experimenter and the child each had a card fastened to a staff like a flag. Each also had a screen behind which the card could be readily placed out of view. The experimenter gave the instructions in the form of a simple and direct command requiring the child to observe how long she showed her card and then show his card for exactly the same length of time. Two trials were made for each interval in the double fatigue order. After the regular series of trials, the child's conception of a common time-interval was measured by requiring him to show his card "exactly one-half minute." As there is no constant variation with sex, the records for the boys and the girls are given together in Table XVIII.

The illusion is of the same nature as for the students, but it is stronger for the children. It decreases with increase in

the age of the children. This is primarily an illusion of childhood. It is easy to see how the feeling of suspense may lead them to terminate the interval prematurely. The error in the estimation of an absolute interval is much greater than the error in the reproduction of a given standard interval. To the children, one-half minute means less than a quarter of a minute.

TABLE XVIII.

Time-estimate by Children.

Age	*n*	*5*	*d*	*10*	*d*	*20*	*d*	*X*	*d*
6	19	5	2	6	2	10	4	9	6
7	19	4	1	6	2	10	4	10	5
8	23	4	1	7	2	14	5	13	7
9	16	4	2	8	3	13	4	12	4
10	15	5	2	8	2	15	4	17	6
11	23	5	2	8	2	18	4	16	6
12	20	5	1	8	2	14	3	13	4
13	20	5	1	9	2	16	3	13	3
14	19	5	1	8	1	15	3	12	4
15	13	4	1	9	1	17	4	18	6
Average		5	1	8	2	14	4	13	5

Unit of measurement, 1 sec.
n, number of children, boys and girls taken together.
d, mean variation.
X, free estimate of one-half minute.

To determine whether the bright children are subject to these time-illusions to a less extent than the dull children, the classification of the children according to general mental ability was collated with the classification according to the degree of illusion, by the method illustrated on p. 59. There appears to be no functional relation between the two processes.

ON THE ANALYSIS OF PERCEPTIONS OF TASTE

BY

PROFESSOR G. T. W. PATRICK.

The following is an experimental study[1] designed to contribute to the further analysis of so-called perceptions of taste into their constituent sensations. It is based upon a series of tests made with an anosmic observer in tasting a considerable number of common articles of food and drink. The account of the experiments themselves is preceded by a brief discussion of the psychology of taste as at present understood.

Taste perceptions are, for the most part, complexes having as their constituent elements sensations of taste, smell, touch, temperature, sight, and muscle sensations, but no exact analysis of such perceptions has ever been undertaken. Popularly taste perceptions are supposed to be made up largely at least of taste sensations, and we speak of the "taste" of cheese, milk, wine, tea, coffee, chocolate, strawberries, peach, apple, beef, mutton, turkey, oysters, etc., as well as of sugar, salt, and vinegar. In the same way all volatile substances which are perceived through the nose are popularly called odors, and we speak of the smell of ammonia, menthol, acetic acid, as well as of violet, wintergreen, and camphor. If we turn from the popular view to the sciences of physiology and psychology, we find only a partial escape from this confusion. Despite the recent valuable researches of Kiesow upon the sense of taste and the work of Zwaardemaker on the physiology of smell, both the physiology and psychology of these senses are in a very undeveloped condition, particularly as

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[1] A preliminary report upon some of the experiments included in this article was made at the meeting of the American Psychological Association, at New York, December, 1898.

regards our knowledge of their qualitative differences. We learn that sensations of smell are indefinite in number and roughly and unsatisfactorily classifiable into nine or ten classes.[1] Whether these many odors are unanalyzable simple sensations or complexes of a few elementary odors is not known. Nagel, from recent experiments in mixing odors, thinks it probable that our common perceptions of smell are fusions of a certain number of elementary odors, but these he does not attempt to name.[2]

As regards taste, physiology and psychology teach us hardly more. We learn that there are only four simple tastes, sweet, bitter, salt, and sour; that these elementary taste sensations do not stand in any determinable relation to each other except that they present some of the phenomena of compensation and contrast; and finally that tastes and odors are constantly confused in experience. If we read further in the text books, we find that there is much confusion as to the composition of the taste perceptions of common experience. Instead of four simple tastes, we are told presently that there are a vast and indefinite number, and that sweet, bitter, salt and sour are merely four *classes* into which all tastes may be conveniently divided. For instance Baldwin says: "Tastes are infinite in their variety and cannot be classified. Certain classes of tastes are well discriminated in experience, such as sweet, bitter, sour; but they are very few compared with the vast number which remain undescribed."[3] Foster says: "We recognize a multitude of distinct tastes, which may be broadly classified into acid, saline, bitter and sweet tastes."[4] Wundt says: "We can distinguish four distinct primary qualities. Between these there are all possible transitional tastes, which are to be regarded as mixed sensations."[5] To quote finally from a

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[1] Compare ZWAARDEMAKER, Die Physiologie des Geruchs, XIII.

[2] *Ueber Mischgerüche und die Komponentengliederung des Geruchssinnes.* Zeitschr. f. Physiol. u. Psychol. d. Sinn., 1897, XV, 82.

[3] Hand-book of Psychology: Senses and Intellect, p. 87.

[4] Text-book of Physiology, Fifth edition, Part IV, p. 221.

[5] Outlines of Psychology, Eng. trans., p. 53.

recent valuable monograph by Dr. Kahlenberg, he says: "The sensations of taste are commonly classified as those of sweet, sour, salty, and bitter. * * There can be no doubt, however, that there are very many kinds and shades of taste that are quite distinct and not to be referred to sensations of touch, and that the above classification can claim at best to be only a very rough one. The investigator of this subject is soon struck by the fact that we have so few names to describe the various tastes."[1]

Now there are several interpretations that might possibly be given to these expressions, all of them perhaps representing views that are freqently held as to the composition of taste perceptions. The first is the one already referred to, that sweet, bitter, salt, and sour are merely classes of tastes based on four kinds of resemblances among an indefinite number of qualitatively different taste sensations. This view, I think, is not seriously held by any physiologist or psychologist. The tendency has been towards a constant decrease of the number of qualitative differences, from an indefinite number, first to ten, then six, then four, and finally by some to two. The second view is that the four elementary tastes have merely a physiological basis in four different kinds of nerve endings, like the three or four hypothetical elementary processes in the theories of color sensation, and that an indefinite number of tastes may result from the proportions in which these elementary processes are set up. We might thus have any number of transitional tastes like the various color tones of the spectrum. This view also must be rejected, as there is no known ground for it either in anatomy or physiology, or in psychological observation, and it derives no support from experiments in combining simple taste stimuli. A third view is that the indefinite number of tastes are not physiological or psychological compounds, but rather mixtures or fusions of the four elementary taste sensations, analogous perhaps to the fusion of tones in musical clangs. A fourth view is that there are not

[1] *The Action of Solutions on the Sense of Taste.* Bulletin of the University of Wisconsin, 1898, No. 25, p. 11.

an indefinite number of tastes at all, but only four. To this hypothesis I shall return below.

As regards the third view, it is perhaps the one which most of the writers quoted above would endorse. However, it presents not only serious theoretical difficulties, but seems to lack clear experimental proof. If we omit quantitative relations, this theory would not give us a vast and indefinite number of tastes but only *fifteen*, for from four elementary tastes only eleven combinations can be made, admitting groups of two, three, and four; and even if the components of the groups might be quantitatively varied, it would seem that we should in experience have a few ever recurring unique and well recognized tastes, as for instance a special taste resulting from the sour-salt fusion, or from the bitter-sour-sweet fusion. But the tastes of common experience are not at all of this kind. Experiments which I have made in the mixing of simple solutions of sweet, bitter, salt, and sour substances have failed to reveal clearly tastes which can properly be called fusions of simple tastes. The observer may say that he tastes a mixture of sweet and sour, where psychologically there are present two simple sensations referred to a mixture of two solutions. The musical clang can only be analyzed in perception by the trained ear and then not always or fully. In any case it has a distinct unity and individuality. Omitting for the moment the possible exceptions described by Kiesow and to be discussed below, no such fusion can be obtained in mixing sweet, bitter, salt, and sour. The solutions are not only analyzable but they are described as "sweet and sour," "salt and bitter," etc.

As a preliminary experiment, I prepared the following standard solutions: Cane sugar, 40 per cent; sulphate of quinine, 0.125 per cent; table salt, 5 per cent; tartaric acid, 5 per cent.[1] These were then combined in equal amounts in the eleven com-

[1] The above percentages for the several substances were chosen because they seem by rough tests to give solutions which, psychologically considered, are of about equal strength. Among these four substances there is certainly not an entire absence of chemical action, as for instance between the salt and tartaric acid, but with these strong solutions the chemical action is not sufficient to affect materially the results.

binations possible with groups of two, three, and four. In addition to these eleven combinations, six other combinations of salt and sugar were made with quantitative variations, with the purpose of determining whether different proportions of simple tastes might combine to produce a new taste. Four observers, two men and two women, tasted the mixtures. The method of procedure was the same as that described in detail below in connection with my other experiments, except that the observers were not blindfolded. The solutions were in glass stoppered bottles marked merely with a code number. The instructions given to the observers were to taste the solution as carefully as possible, swallow it, and observe whether it was sweet, bitter, salt, or sour, or a combination of these, or whether in particular any other taste than these appeared, then, to write the result after the code number of the solution upon paper provided for each observer. The amount of each mixture tasted was about one ccm. taken from a silver spoon. The results of the experiments are exhibited in Table A. In the

TABLE A.

SOLUTION.	MRS. S.	MR. S.	MISS W.	MR. E.
Sweet and sour	Sweet and sour	Bitter.	Sour.	Sour and (sweet)
Sweet and salt.	Sweet.	Sweetish, with an after salty taste.	Sweet and salt.	Salt and sweet.
Salt and sour.	Sour and salt.	A salt and bitter taste combined. The salt first noticed.	A salt, the like of which I never tasted.	Very sour. Does not seem to be simple.
Sweet, salt and sour.	Sour and salt and sweet.	Bitter, followed by an astringent effect.	A peculiar sour, not sweet or salt or bitter.	Sour and salt. Not same as before. Perhaps something more.
Salt and bitter.	Bitter.	A bitter of quinine, very decided.	Bitter.	Bitter and (salt)
Sweet, sour, and bitter.	Bitter and sour.	A bitter, followed by a possible sweetish taste	Bitter and salt.	Sour and bitter.

TABLE A. Continued.

SOLUTION.	MRS. S.	MR. S.	MISS W.	MR. R.
Sweet, salt, sour, and bitter	Bitter, sour, and sweet.	Bitter.	Bitter and sour.	Sour and bitter.
Sweet, salt, and bitter.	Sweet and bitter.	A sweet, decidedly disagreeable bitter.	Bitter and sweet and salt	A faint bitter and sweet.
Sweet and bitter.	Sweet and bitter, sour.	A sweet bitter taste.	Bitter and sweet.	Sweet and bitter.
Sour and bitter.	Bitter and sour.	Bitter.	Bitter, but not quinine. Puckering, like bark.	Bitter, sour and pungent.
Salt, sour and bitter.	Sour and salt.	A sour, bitter taste.	Salt and sour, yet different.	Sour and (salt)
Sweet, 1 part. Salt, 2 parts.	Salt and sweet.	A salty, sweet taste.	Salt and sour.	Sugar and salt.
Salt, 1 part. Sweet, 2 parts.	Sweet.	Sweet.	Sweet and salt, (sour).	Salt and sweet.
Salt, 1 part. Sweet, 4 parts.	Sweet.	Sweet.	Sweet and sour.	Salt and sweet.
Salt, 1 part. Sweet, 3 parts.	Sweet. There is something else. Cannot say what.	Sweet.	Sweet.	Sweet and salt.
Sweet, 1 part. Salt, 4 parts.	Salt and slightly sweet.	Salty.	Salt.	Salt and (sweet)
Sweet, 1 part. Salt, 3 parts.	Salt and sweet and bitter.	A sweetish salty taste.	Salt and sour. A seemingly new combination.	Sweet and (salt)

column under the name of each observer is given his judgment upon each mixture in his own words. We notice, first, that in the sixty-eight judgments included in the tests no new tastes appear. The distinctness or intensity of the sensation varies, but it is always "sweet," "salt," or "sour," or "sour *and* salt," "sweet *and* bitter," etc., although one observer speaks of "a peculiar sour." There are only three exceptions to this. In one case we find the word "astringent," in one case,

"puckering," and in one case, "pungent." These, however, are not tastes, but touch sensations. In the second place, we notice a considerable facility in analyzing the mixtures. Those containing two ingredients are usually correctly analyzed, and those containing three are sometimes correctly analyzed, while without exception every ingredient of every mixture is detected by some of the four observers. The one exception is the mixture containing all four ingredients, where the salt is not detected by any observer. In the combination of sweet and salt in different proportions, we notice that when one or the other is proportionally increased, the observer may detect this alone, but that such a mixture does not result in a qualitatively new taste. This experiment I supplemented by another, taking the standard solutions of different strengths from those in the first experiment and combining them in all the possible ways. It is not necessary to report this experiment in detail, but the results agree with those of the first test.

These experiments, preliminary and incomplete, of course, contribute nothing, therefore, to the support of the third view mentioned above, viz., that the numerous tastes of common experience are mixtures or fusions of four elementary tastes. Thus far there does not appear to be anything like a fusion of tastes comparable to the fusion of tones or colors, which could give us tastes different from the four simple tastes, as white is different from red or green, or as the musical clang is different from the tones which compose it.

Kiesow, in one of the series of interesting and valuable researches upon the taste sense which he has published in Wundt's *Studien* and elsewhere, has considered this problem of the fusion of simple taste sensations,and as a result of his experiments has come to a conclusion which seems to be somewhat different from the one just suggested.[1] As a result of a series of experiments in the mixing of tastes, he concludes that the sense of taste is not like that of smell, where the mingling of different stimuli gives the phenomenon of rivalry rather than

[1] *Beiträge zur physiologischen Psychologie des Geschmackssinnes.* 4. *Compensations- und Mischungserscheinungen.* Phil. Stud. (Wundt), XII, 254.

of fusion, and that in this respect the sense of taste is more comparable with sight and hearing. He finds that under certain conditions simple taste qualities, such as sweet and salt, unite to form a mixed sensation, which is a qualitatively new or ground sensation, in which sometimes at least the elementary tastes may be distinguished as we distinguish the elements of a musical clang or of a mixed color like brown.[2] Kiesow finds the best experimental illustration of this in the mixture of sweet and salt. In certain proportions these neutralize each other as such and produce a new sensation which is called insipid, or alkaline, or insipid-alkaline, ("fade," "laugig," "laugigfade"). He tried among others not reported in detail, twenty-five combinations of cane sugar and salt, the solutions combined varying in strength from 1 per cent to 40 per cent. These mixtures were tried upon himself as observer. Of the twenty-five, two gave no taste except insipid or alkaline, three others gave an insipid or alkaline taste in connection with sweet or salt, while all the rest gave either a sweet or salt or a strong preponderance of one or the other.

It is not at all the primary object of the present paper to discuss this problem and it was not possible for us to try any experiments bearing directly upon it which should have anything more than a suggestive value, though it seems to me very desirable to repeat these experiments using a larger number of observers and trying all sorts of combinations of the elementary tastes. Kiesow's results suggest two questions: First, may not the flat, insipid, and alkaline tastes which he observed be otherwise explained than by a fusion or mixture of sweet and salt? Second, even if they are to be so explained, can we hope to find in such fusion any results at all adequate to account for the "infinite variety" of tastes of which some psychologists speak? As regards the first question, I have found that observers tasting distilled water, frequently give the judgment "flat," "insipid,"

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[2] A statement of this view adopted from Kiesow may be found in WUNDT'S Outlines of Psychology, Eng. trans., p. 53.

or "alkaline," and if a weak solution of salt in distilled water is used, the alkaline taste is not unusual. It seems to me highly probable that the alkaline taste that Kiesow got from a mixture of sweet and salt was due to the salt alone. A solution of salt, as much weaker than the one used as the amount of neutralization effected by the sugar, might give the same sensation. I have observed a peculiarity about the taste of salt to which I think attention has not sufficiently been called. In experiments upon minimal tastes made some years ago, Nichols and Bailey reported that salt was tasted by male observers in the proportion of 1 to 2240.[1] While Nichols and Bailey's results in respect to sweet, bitter, and sour have been somewhat closely confirmed by subsequent experiments, those respecting salt have not. Salt may be recognized by a few observers in the proportion of 1 to 600, but more commonly the proportion is 1 to 400 or 1 to 300. Looking for the explanation of this discrepancy, I found that, while with sweet, bitter, and sour, the threshold of recognition coincides nearly with the threshold of sensation, this is not the case with salt.[2] If a solution of salt is made of the strength of 1 to 2000, it can be distinguished constantly, at least by some observers, from distilled water, but it cannot be recognized as salt. The taste is sometimes called alkaline. One of my observers (See Table B.) beginning with a solution of salt in the proportion of 1 to 1100, called the taste alkaline continually until the proportion of 1 to 300 was reached. This circumstance may explain Nichols and Bailey's results and it may also explain the alkaline taste of Kiesow's mixtures of salt and sweet. As for the explanation of the circumstance itself, the hypothesis may be made that the small amount of salt in the solution is sufficient to affect the end

[1] *The Delicacy of the Sense of Taste.* Nature, XXXVII, p. 557.

[2] Kiesow refers to the difference between the threshold of sensation and that of recognition in the case of sweet, salt, and sour, but makes special reference to it in the case of salt. He is discussing the general fact that all taste sensations are accompanied by touch sensations. See Wundt's *Studien*, X. 525, 531. I have not found that the above difference is appreciable in the case of sweet, bitter, and sour.

organs of touch but not those of taste, the alkaline "taste" being really a touch sensation. Indeed, there are several reasons for thinking with Valentin and others that salt and sour are not true tastes at all. Kiesow's experiments with cocaine, however, seem to substantiate his position that there are four tastes, but that salt and sour in particular are attended by touch sensations.[1]

Returning to Kiesow's conclusions about mixed tastes, I attempted to verify them by repeating some of his experiments. I selected from his list those mixtures of sweet and salt which gave, according to him, most decidedly the new tastes, that is, the insipid and alkaline tastes. These were cane sugar, 1 per cent, and salt, 1 per cent, mixed in the proportion of 50 to 25; cane sugar, 2 per cent, and salt, 2 per cent, mixed in the proportion of 50 to 20; and cane sugar, 4 per cent, and salt 4 per cent, mixed in the proportion of 50 to 10. I tested four observers, two men and two women, with these solutions. They were blindfolded and ½ ccm. of the solution was placed upon the tongue by means of a glass dropper. They were instructed to taste the material carefully and swallow it. They were given a second trial if they desired. They wrote their judgments upon prepared slips, being simply instructed to name the taste if any, whether sweet, bitter, salt, sour, alkaline, or metallic, or any other taste. The results of the test are exhibited in the first part of Table B. The three solutions mentioned above are indicated in the table by the letters a, b, and c. Of the twelve judgments concerning these solutions, three revealed sweet and salt; five, salt; two, sweet; and two, a slight unrecognized taste. No observer pronounced the taste in any case alkaline, flat, or insipid. To complete the experiment, I prepared solutions of salt ranging in strength from 1 to 1100 and 1 to 200, and with these I tested the same observers. The results are exhibited in the second part of Table B. With one

[1] *Ueber die Wirkung des Cocain und der Gymnemasäure auf die Schleimhaut der Zunge und des Mundraums.* Phil. Stud. (Wundt), IX. 523. Compare idem. X. 524.

TABLE B.

SOLUTION.	MRS. S.	MISS W.	MR. S.	MR. E.
a Cane sugar, 1% 50 parts. Salt, 1% 25 parts	Slight taste, but do not recognize it. Might be distilled water.	Very weak, possibly the faintest suggestion of salt.	A slight taste. I believe slightly sweet.	A taste, but I can't tell what.
Distilled water.	Water.	Same as above, only a little stronger.	Distilled water.	No clear taste.
b. Cane sugar, 2% 50 parts. Salt, 2% 20 parts	Slightly salty.	A little more salty than the preceding.	A salty taste, but very slight.	Salt, perhaps something more.
Distilled water.	Water.	Very like the first. May be a little sweet.	No taste. Water	No taste.
c. Cane sugar, 4% 50 parts. Salt, 4% 10 parts	A salt, sweet taste.	Seemingly a mixture of salt and sweet, but weak.	Sweet, very distinct.	Salt and sweet.
Distilled water.	Water.	Very like water No other taste	Distilled water.	Bitter.
Salt, 1-1100.	No taste.	No taste. Like water.	Slightly alkaline. A trifle like distilled water.	A taste, but I cannot tell what.
Distilled water.	No taste.	No taste. Like water.	I believe it is distilled water, though it seems to be alkaline.	No taste.
Salt, 1-1000.	No taste.	It is like water, but there is a taste.	I believe it is distilled water, although it seems to be alkaline.	Bitter.
Distilled water.	Water.	Same as preceding.	Distilled water.	Bitter and something more.
Salt, 1-900.	Slight taste, but do not know what it is.	Same taste as preceding, but stronger.	Distilled water.	No taste.
Salt, 1-800.	No taste.	Same as preceding.	A slight taste. Do not know what it is.	No taste.

TABLE B. Continued.

SOLUTION.	MRS. S.	MISS W.	MR. S.	MR. E.
Distilled water.	Water.	Same as preceding	No taste. Water	No taste.
Salt, 1-700.	Slightly alkaline.	A little stronger. Possibly some salt. Too faint.	Slightly alkaline, but may be distilled water.	No taste.
Salt, 1-600.	Some taste, but do not know what.	Same as preceding. Can't recognize the taste.	No taste. Distilled water.	May be salt (?)
Distilled water.	Water.	Same as preceding.	Distilled water	No taste. Might be bitter.
Salt, 1-500.	Alkaline.	Same as preceding, but stronger.	Seems slightly alkaline; may be distilled water	No taste.
Salt, 1-400.	Some taste, but do not know what.	Stronger than preceding. Possibly salt.	Slightly alkaline, I think.	Salt.
Salt, 1-300.	Alkaline.	Still stronger than preceding. Same taste.	Salty, *sure*.	Mixed taste. There is salt in it.
Distilled water.	Water.	Same as preceding.	Slightly alkaline. Probably distilled water.	May have some taste.
Salt, 1-200.	Salty.	Same as preceding, but stronger.	Salty, *sure*.	Salt.

observer, a suspicion of salt begins with the 1 to 700 solution. With a second it begins at 1 to 600. The third recognized it at 1 to 300, and the fourth at 1 to 200. But the alkaline taste, although it failed in my tests to appear at all with the sweet-salt solutions, appears now eight times with the pure salt solutions and twice with distilled water. It seems to me doubtful, therefore, although this single experiment of mine can have only a suggestive value, whether the alkaline taste is a mixed taste made up of salt and sweet as indicated by

Kiesow and as suggested by Wundt.[1] As regards the second question mentioned above, even if the mixture of salt and sweet should be found to produce a new neutral sensation, such as insipid or alkaline, it would seem that the experimental results of such mixtures or fusions have thus far been too meagre to encourage us to look in this direction for the source of the multiform "tastes" of common experience.

To gather up the results of this preliminary survey of the psychology of taste, I may say that the hypothesis which seems at present most in accord with known facts is that there are only four taste sensations (possibly only two); that these remain distinct in consciousness, not subject to fusion or mixture with each other; and that the manifold taste perceptions of daily experience are made up of these four taste sensations with their grades of intensity, and sensations of smell, touch, temperature, sight, and muscle sensations. The fine discriminations of foods and drinks, called "tastes," are due to the delicate sense of touch possessed by the tongue and (as has long been known) to smell. Of these two perhaps the touch sensations are more important, while sight sensations, as will appear below, play a more essential part than has commonly been supposed. The taste sensations themselves have a comparatively unimportant role so far as perception is concerned. They have little to do with discrimination. Their affective value, however, is great. Sweet things are "good" and bitter things are "bad." To take an illustration at random, honey and all the different kinds of syrups and molasses have only one taste, viz., sweet, and indeed to a child of six years they are all alike "good."[2] The peculiar "flavor" of the syrups and molasses is due in this case to smell. The pleasant maple "taste" of pure maple syrup is an odor. It is only necessary to close the nose aud take some maple syrup upon the tongue, without swallowing, to show this. It is merely sweet and smooth and has a certain amount of stickiness and viscos-

[1] Outlines of Psychology, Eng. trans., p. 53.

[2] An exception may be found in New Orleans molasses which has a slight bitter taste together with the sweet.

ity. Swallow it or release the nose and the peculiar maple flavor comes out in a striking manner. To some extent, however, the various syrups and molasses may be distinguished without the sense of smell by touch and the muscle sense, owing to their different degrees of smoothness and viscosity. (See below, Table C, Sec. IX.) It is in this way probably that honey may be distinguished from the syrups. Its consistency and its absence of odor, rather than the intensity of its sweetness, are its marks. Without sight, however, as will be seen from Table C, honey is by no means easily distinguished from other sweets, especially if it is made as limpid as possible by heating.

The perpetual confusion of tastes and odors is much increased by the so-called gustatory smelling. Particles from the masticated food or from fluids taken into the mouth, ascend on either side of the soft palate through the pharynx and the posterior nares to the olfactory region, an action greatly facilitated, of course, in swallowing. By tightly closing the nostrils, the air currents are precluded, and in this way very satisfactory experiments may be instituted in eliminating smell sensations from taste perceptions. This, however, can best be attained in the case of anosmics.

The analysis of taste perceptions into their respective elements is a matter of considerable difficulty. Sight may be eliminated by blindfolding the observer, and smell by experimenting with an anosmic subject. In the experiments reported in this paper, I have in this way satisfactorily eliminated these two sets of elements. The elimination of touch and temperature sensations is more difficult. Cocaine removes sensibility to taste as well as touch.[1] Gymnemic acid which destroys for hours the sensibility to sweet and partly to bitter, might perhaps be used upon a blindfolded anosmic observer with interesting results. In the following experiments, the only attempt made to eliminate touch and temperature sensations was

[1]See KIESOW, *Ueber die Wirkung des Cocain, etc.*, Phil. Stud. (Wundt), IX. 4.

by presenting the substances in a uniform fluid condition of the same temperature. This, of course, was not always possible and when possible not always effective. With many substances a change of temperature is accompanied by a change of texture or viscosity, and the observer makes a conscious or unconscious allowance for such change. If for instance one attempts to prevent the identification by texture of butter or honey by heating them, the observer, perceiving the unusual temperature, makes allowance for the decreased viscosity. (See Table C, Sec. IX.)

Anosmia, or absence of the sense of smell, may be partial or complete. Cases of partial anosmia, like cases of color-blindness, are valuable for the aid they may give in the settlement of problems concerning sensations of smell. Cases of complete anosmia are valuable in separating sensations of smell proper from those caused by mere irritation of the nasal membranes, and particularly for separating sensations of taste from those of smell with which they are so often confused. Cases of complete anosmia are rare and have been little used for psychological purposes. One case has been reported by Jastrow[1] who made some interesting researches in connection therewith, and other instances have been reported in medical journals. A case of complete anosmia having come to my attention some months ago, a series of experiments was begun for the purpose of making some contribution to the problems before us. The observer, Mrs. S., is a very good one for such researches. She is a married woman, twenty-six years of age, of education and refinement. Having made formerly a special study of chemistry, she is familiar with the names and tastes of ordinary substances such as would be used in these researches. Being a housekeeper of experience, she is acquainted with the names and tastes of the materials of the common foods and drinks. According to her own testimony, she is almost entirely, if not entirely, devoid of the sense of smell and has always been so. She recalls that as a young girl she

[1] American Journal of Psychology, Vol. IV. p. 407.

suffered disappointment because she could not smell the fragrance of flowers as the other girls said they did. Her anosmia may therefore be congenital, but her family history reveals no other defect of this kind and it is much more probable that the defect dates from a severe attack of scarlet fever which she had at the age of five. Rhinoscopic examination by a specialist revealed nothing abnormal in the appearance of the olfactory region. My own conclusion is that the case is one of complete anosmia of the intracranial kind. The completeness of the anosmia was, however, determined by the following experiments. The observer was tested with a large number of odorous substances representing all the nine classes mentioned by Zwaardemaker.[1] She was blindfolded and the odorous substances were held under the nose while she tried to detect the odor by sniffing. Care was taken to avoid fatigue by having many sittings and by repeating many of the substances upon different occasions. The following substances gave no sensation or reaction whatever: Absolute alcohol, methyl alcohol, butyric acid, acetone, orange, sweet orange peel, oil of lemon, oil of tansy, oil of spike, peppermint, spearmint, origanum, cardamon, anilin, oil of cloves, wintergreen, pennyroyal, lavender water, camphor, tincture of arnica, thyme, oil of cinnamon, oil of mustard, Java and Mocha coffee and black and green tea (these both powdered and in infusions), rosemary, cinnamon, mustard, mace, pepper, allspice, cloves, ginger, vinegar, thymol, oil of bitter almond, extracts of white rose, heliotrope, violet and verbena, vanillin, gum benzoin, turpentine, acetamid, musk, raw onion, asafoetida, javelle water, hydrochloric acid, tincture of iodine, valerianic acid, india rubber, iodoform, gum ammoniac, gum myrrh, carbolic acid, napthaline, vaseline, resorcin, benzol, benzine, naptha, Venice turpentine, diphenylamine, tobacco, tobacco smoke, burnt cheese, lactic acid, rancid lard, paregoric, laudanum, dried blood, decomposing animal matter. The following substances gave a reaction: Ammonia, ammonia sulphide,

[1] Physiologie des Geruchs, p. 216.

aromatic spirits of ammonia, acrolein, acetic acid, carbon disulphide, sulphurous oxide, ether, chloroform, bromine water, menthol, and pyridin. Oil of cloves upon second trial gave a faint sensation "far back in the throat." While at least some of the last named substances have odors, their characteristic reaction is not that of smell. They affect either the end organs of taste, being like chloroform very volatile and reaching the end organs of taste about the soft palate through the nose, or they are pungent and attack the membranous linings of the nasal cavities, stimulating in this way branches of the fifth nerve. For instance, menthol produced a sharp reaction with the observer, not being recognized, however. But menthol when held near the eyes produces a painful reaction. The action of ammonia and many of the ammonia compounds is similar, as is well known. Chloroform produced what the observer called a pleasant sweet sensation, felt in the back part of the mouth, which was of course its taste, it having a sweet taste. The character and reaction of ether is similar. Pyridin produced a marked and very unpleasant sensation located in the mouth. If held to the open mouth of the observer, the sensation was the same but stronger. Pyridin is a strong organic base very volatile and very miscible with water. The fumes of pyridin pass through the nose and diffuse over the tongue mixing with the fluids of the tongue and producing a true taste, combined probably with touch sensations. A normal observer may test this by holding a small bottle of pyridin under the tip of the tongue with the mouth closed tightly around the tongue and the nose closed. The unpleasant effect is very apparent. The substances mentioned in my second list may, then, be regarded as samples of a rather large class of volatile substances which give rise to touch and taste sensations commonly mistaken for odors.

This preliminary experiment was followed by others. I tested next the observer's sensibility to pure taste stimuli, using four normal women as control observers and comparing the results with such published records as we have concerning normal taste sensibility. Solutions of cane sugar, common

salt, tartaric acid, sulphuric acid, sulphate of quinine and strychnine were used. The following table exhibits a summary of these results:

Solution.	*Mrs. S.*	*Four Women.*	*Forty-six Women.* (Nichols & Bailey.)
Cane sugar	1—150	1—144	1—204
Salt	1—200	1—675	(1—1980)
Sulphuric acid	1—2000	1—2368	1—3280
Tartaric acid	1—1000	1—1500	
Quinine sulphate	1—160000	1—640000	1—456000
Strychnine	1—1000000	1—1000000	

From these experiments it appeared that my anosmic observer's sensibility to taste is slightly below the average sensibility of other women, although hardly enough tests upon normal women have been made for purposes of comparison. Certainly Mrs. S's sensibility to taste is not above the average, as would be expected if taste sensations play any important part in taste perceptions.

Experiments were made upon the observer's sensibility to touch and temperature stimuli. The pressure sense upon the fingers, hands, and face was found to be normal. Space discrimination upon the hands and fingers was also normal, as was the temperature sense. It was thought that the absence of the observer's sense of smell, since no superiority in the sense of taste was found, might find its compensation in an unusual fineness of the touch sense upon the tongue. A series of experiments was therefore made upon the active and passive touch sense of the tip of the tongue. It was possible to make the same tests upon only two other female observers and the results therefore can claim little value. They showed, however, a decided superiority on the part of the anosmic observer in respect to fineness of discrimination in passive touch, but no superiority in active touch.

The experiments in taste to which the above mentioned tests were all preliminary were begun in October, 1898, and continued for about eight weeks. About 200 substances, for the most part common foods and drinks, were tried with the anosmic observer and simultaneously with two (or three) con-

trol observers. Three married women, between twenty-five and thirty-five years of age, acted in the latter capacity, only two of them serving at a time. They were all very bright women of good education, and at the same time experienced housekeepers familiar with the taste and smell of all ordinary articles of food and drink and having a knowledge above the average of flavoring extracts, fruits, spices, etc. The observers met for the experiments from one to three times a week. The sittings lasted from one to two hours. During this time from fifteen to twenty substances were tasted. Although there were periods of rest during the experiment, it was at first thought that the last tests in each sitting would be unreliable owing to fatigue, and they were accordingly repeated at the next sitting. This was found to be unnecessary. The substances were so different in character and for the most part the tastes and odors were so mild, that there were no disturbing influences from fatigue. The method of procedure at each sitting was as follows: The three observers, the anosmic and the two normal observers, sat around a large table. They were carefully blindfolded and each provided with a silver teaspoon, a glass of lukewarm distilled water, and a lead pencil. After each taste the mouth was rinsed with the distilled water. There was also for each observer a beaker for spitting out the water or the substance tasted when the latter was necessary. Usnally, however, the substance was swallowed, as in this way the observer had the greatest possible advantage in tasting and smelling. Three sets of cards were prepared beforehand, one for each observer, with her name and the name of the substance to be tasted. An assistant handed the appropriate card to each observer, and after tasting she wrote upon it her judgment. She was required to name the substance if she could; if not, to name or describe the taste; and if she could not do this, to write whether it had any taste or not. The amount offered was from one-half to one teaspoonful. It was handed to the observer, who smelled it as much as she wished and then tasted it as carefully as she could. She was not required to take the whole amount, for it was found that with many

substances a small amount gave the best results. This could always be wisely left to the judgment of the taster. The observers were not allowed to talk during the experiments, nor to discuss the tastes afterwards, and were not told as to the accuracy of their judgments. As far as possible, and except where noted in the tables, the substances were presented at a like temperature, about 25° C. Exceptions to this rule are noted in the tables. In all cases where it was possible, the substance was presented in liquid form, or in both liquid and solid forms. The object of this was to reduce to a minimum the action of the muscle and touch sensations connected with the tongue. For instance, meat broths may be prepared having very nearly the consistency of water, and certain vegetables, such as cabbage and onion, may be separated from the water in which they are boiled and the latter used in taste experiments. These devices, however, apply to only a limited number of the substances tried and in many cases they are not wholly effective in eliminating the touch and muscle sensations.

In the table which follows, the judgments of Mrs. S., the anosmic observer, are for convenience of comparison printed in italics. The three control observers appear as Mrs. R., Mrs. K., and Mrs. L. On one occasion (See Section VI), neither Mrs. K. nor Mrs. R. being available, another woman, Mrs. O., was substituted. Mrs. O. is a housekeeper noted for her skill in cooking. On one other occasion (See Section V), there was an additional male observer, Mr. S., a married man 30 years of age, instructor in the University. In blindfolded tasting some proficiency is gained by practice, or at any rate, one or two hours' practice is necessary before the observer becomes accustomed to the conditions so as to give the best results. This may account for the somewhat unsatisfactory results exhibited in Section I, and it may also explain in part the rather wild judgments of Mrs. O. and Mr. S. upon the two occasions when they were introduced into the experiment. The effect of merely eliminating sight sensations in tasting is striking enough with Mrs. R., Mrs. K., and Mrs. L.; it becomes extreme

with Mrs. O. and Mr. S. Making some allowance for lack of practice in the case of these two observers, I am disposed to think that my three control observers, Mrs. R., Mrs. K., and Mrs. L., possess a power of discrimination in tasting somewhat above the average. The substances which appear in Sections I and II were nearly all obtained from a druggist, the fruit syrups, however, having been prepared from pure fresh fruits. The substances which appear in all the other Sections were procured by the writer either in the market or at his own home and were known to be pure. It may be added that in a few instances Mrs. S. was asked to say what her judgment was based on, the purpose being to ascertain whether she depended upon the taste or the texture of the material. This may explain the form of certain of the answers.

TABLE C. SECTION I.
October 26 and 29, 1898.

SUBSTANCE.	MRS. S.	MRS. R.	MRS. K.
Normal alcohol.	*Like maple sap.*	Like brandy or alcohol.	Alcohol.
Raspberry syrup.	*Honey.*	Raspberry.	Currant juice.
Distilled water.	*Water.*	Water.	Water.
Tincture of licorice.	*Licorice.*	Something like rhubarb or honey	Licorice.
Distilled water.	*Water.*	Like water.	No taste.
Peach syrup.	*Sugar syrup.*	Like syrup.	Quince jelly.
Normal alcohol.	*Like water.*	Do not know.	Alcohol.
Tincture of celery	*Alcohol.*	Bitter like quinine	Acid and nauseating.
Distilled water.	*Water.*	Like water.	Like thickened water.
Tincture of vanilla (with sugar).	*A sweet hot taste; I can think of nothing like it.*	Like vanilla.	Vanilla.
Strawberry syrup	*Sweet taste like honey.*	Like peach syrup.	Sugar syrup.
Pineapple syrup.	*Sweet.*	Like pineapple syrup.	Pineapple.

TABLE C. SECTION I. Continued.

SUBSTANCE.	MRS. S.	MRS. R.	MRS. K.
Distilled water.	*Water.*	Like water.	Water thickened.
Oil of rose and alcohol.	*Certainly one of the alcohol series, but do not know which*	A perfume. I can't tell.	Very bitter, but I do not know what it is.
Spirits of almond	*Some spirit or alcohol.*	Like almond extract.	Almond. Told by smell before tasting.
Spirits of Camphor.	*Camphor.*	Like camphor.	Camphor.
Spirits of wintergreen.	*Might be cinnamon oil.*	Like peppermint.	Wintergreen.
Distilled water.	*Water.*	Water.	Thickened water.
Tincture of ginger.	*Like red pepper.*	Quite bitter and spicy.	Pepper.
Spirits of peppermint.	*Peppermint.*	Like peppermint.	Peppermint.
Absolute alcohol.	*A tincture of something sweet and rather volatile.*	Like oil of catnip.	Something hot. Can't tell.
Asafœtida.	*A tincture of something. Might be cloves.*		
Spirits of cinnamon.	*Cinnamon oil.*	Like cinnamon extract.	Cinnamon.
Grape syrup.	*Like grape juice.*	Like strawberry flavoring.	Grape juice.
Ethyl alcohol $\frac{n}{4}$	*Water.*	Lemon.	Alcohol only.
Boiled turnip, mashed and salted.	*Only the flavoring, salt.*	Turnip.	Turnip.
Peach syrup.	*A very sweet taste, but nothing particular to distinguish it.*	Pineapple syrup.	Peach juice.
Rolled oats, porridge, salted.	*Oatmeal, by texture.*	Oatmeal.	Rolled oats.
Boiled potato, mashed.	*Potato.*	Potato.	Potatoes.
Strawberry syrup	*Like the sweet juice of some fruit, but do not know what.*	Syrup	Strawberry juice.

TAALE C. SECTION I. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.
Navy beans, baked and mashed.	*Beans.*	Beans.	Beans.
Cherry syrup.	*A very pleasant delicate taste, but do not know what it is. Sweet, might be fruit.*	Wild cherry syrup.	Almond syrup.
Raspberry syrup.	*A pleasant sweet taste, might be juice of some fruit*	Raspberry syrup.	Fruit juice. Guess apricot.
Distilled water.	*Water.*	Water.	Water.
Oil of catnip, with alcohol.	*Spirits of camphor, I think, but after a time the taste which should be present is absent.*	Bitter, lemon extract.	Don't know. Pennyroyal possibly.
Absolute alcohol.	*One of higher alcohols.*	Alcohol.	Alcohol mixture, can't tell.
Tincture rhubarb	*Bitter, but I cannot even guess what it is.*	Rhubarb.	Rhubarb.
Distilled water.	*Water.*	Water.	Water.
Spearmint, with sugar.	*Very pleasant sweet taste. Delicate enough for a perfume.*	Wintergreen.	Peppermint.
Oil of rose, with alcohol.	*Must be a tincture, but no taste except alcohol.*	Rose extract.	Rose flavoring.
Vanilla extract.	*Some familiar taste, pleasant and spicy, but cannot name it.*		

TABLE C. SECTION II.
November 8, 1898.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. K.
Raspberry syrup.	*Like raspberry juice, by flavor.*	Grape.	Raspberry syrup. Like New Orleans syrup.
Sherry.	*Like a light sour wine, though I never tasted any.*	Spirits, wine. Guess whiskey.	Sherry.
Lactic acid, $\frac{n}{4}$	*Like citric acid, but taste not persistent enough.*	Acid, can't tell.	Lemon and licorice water.
Distilled water.	*Water.*	Water.	Water.
Tincture rhubarb	*Bitter. Do not know anything with which to compare it.*	Rhubarb.	I have smelled this but never tasted it. Is it asafœtida?
Port wine.	*Do not know. Has a warming taste.*	Wine, guess sherry.	Port wine.
Oil of fennel with alcohol.	*A tincture of something that is pleasant. Must have a strong odor.*	Alcohol with familiar flavor. Guess caraway.	Mother Winslow's Soothing Syrup without much sugar.
Distilled water.	*Water.*	Water.	Water.
Vinegar.	*Acetic acid.*	Vinegar.	Vinegar.
Oil of caraway and alcohol.	*Alcohol at least. No other taste.*	Can't tell. Biting, contains alcohol.	Tincture of caraway.
Asafœtida.	*I do not know. It is bitter, probably a tincture.*		Something with onion in it.
Strawberry syrup	*Sweet, thick liquid. I think cane sugar*	Sugar syrup. Can't tell flavor.	Syrup.
Peach syrup.	*Cane sugar.*	Fruit juice. Can't distinguish.	Peach.
Spirits of peppermint.	*Peppermint.*	Peppermint in alcohol.	Peppermint.
Milk, 50 % water.	*Like water with something dissolved, perhaps corn-starch.*	Milk and water.	Milk.
Juice of stewed apples with sugar.	*Peach juice.*	Juice of apple sauce.	Juice of apple sauce.

TABLE C. SECTION III.
November 11 and 12, 1898.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.
Ethyl alcohol $\frac{n}{2}$	*Water.*	Water. Slight taste of alcohol.	Don't know. Water, with dish-watery taste.
Distilled water.	*Water.*	Distilled water.	Water.
Baked squash, mashed.	*Very little taste. Must be some vegetable. Starch taste quite perceptible. From texture should think it might be a squash.*	Baked squash.	Baked squash.
Raw apple, scraped.	*Apple, both by taste and texture.*	Raw apple, scraped.	Apple, scraped.
Milk.	*Corn-starch dissolved in water.*	Milk and water.	Milk.
Beef broth, salted	*Like chicken broth.*	Beef broth.	Chicken soup or broth.
Banana, fresh, crushed.	*Sweet; should say some kind of fruit, but no decided taste.*	Banana, mashed.	Banana.
Distilled water.	*Water.*	Distilled water.	Water.
Canned tomato, cooked, strained.	*Tomato.*	Tomato, strained.	Acid, cannot tell what it is.
Valerianic acid, $\frac{n}{100}$	*Warm water.*	Liquid of little taste. Smells of nuts.	Water, with a mouldy taste.
Lard.	*Lard.*	Fat of boiled beef, or possibly lard.	Lard.
Date, small piece cut from common date.	*Dates.*	Dates.	Dates.
Chicken broth, salted.	*Chicken broth.*	Beef broth, or soup stock.	Chicken or beef broth.
Orange juice, from fresh fruit.	*A very pleasant sour taste, but I do not know what it is.*	Orange.	Orange.
Fig, separated from seeds.	*Prunes.*	Fig.	Fig.

TABLE C. SECTION III. Continued.

SUBSTANCE.	*MRS. S.*	MRS. K.	MRS. L.
Port wine.	*Like weak whiskey.*	Spirits. Sherry?	Port wine. Is there any egg with it?
Distilled water.	*Water.*	Water.	Water.
Cheese, Club House.	*Cheese.*	ClubHouse cheese.	Cheese.
Kerosene.	*It is an oil, but has no taste.*	Kerosene, by smell and taste.	Kerosene oil.
Grape juice and sugar.	*Very pleasant taste. Some kind of fruit juice, but not recognized. Texture might indicate quince, but so sweet, flavor is not distinct.*	Grape juice.	Grape jelly.
Distilled water.	*Water.*	Distilled water.	Water.
Chocolate, boiled in water, unsweetened.	*Bitter and very unpleasant, but I do not know what it is.*	Water with cocoa, slightly greasy.	Chocolate.
Currant jam.	*Quince juice.*	Currant flavor, sugar syrup.	Apple and quince juice.
Cranberry jelly.	*Certainly same as before, which I thought might be quince, but too sweet to get flavor.*	Sugar syrup. Peach or apple flavor.	Fruit syrup, like preserved apple.
Milk.	*Starch water.*	Milk with water.	Milk.

TABLE C. SECTION IV.

November 16, 1898.

SUBSTANCE.	*MRS. S.*	MRS. K.	MRS. R.
Sweet cream.	*Corn starch.*	Cream.	Slightly sweet. I cannot tell what it is.
Vaseline.	*Has no taste, but from texture I should think it might be cooked clear starch.*	Vaseline.	Like a salve.

TABLE C. SECTION IV. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. R.
Distilled water.	*Water.*	Distilled water.	Water.
Claret.	*Unpleasant sour taste, not acid but soured. Do not know what it is like.*	Claret.	Sour, like wine or claret.
Baked apple.	*Flavor of apple, but texture is not of cooked apple.*	Apple sauce.	Apple sauce.
White of hard-boiled egg.	*Very little taste, but by texture and the little taste there is, I conclude it is white of egg.*	Guess some kind of breakfast food.	White part of an egg.
Cabbage, boiled, the liquid only.	*Very unpleasant taste, but do not know with what to compare it.*	Cabbage flavor.	Sweet, tastes like turnip.
Water, undistilled.	*Water.*	Distilled water.	Water.
Preserved quince and apple, juice only.	*Just a sweet liquid with no distinguishing flavor.*	Quince juice.	Sweet, and has the flavor of apple.
Olive oil.	*Thick oil, but has no taste.*	Olive oil.	Some kind of oil; the kind used for sewing machines
Distilled water.	*Water.*	Distilled water.	Water.
Sour milk.	*Do not know; there is a slight taste of grain and sweet.*	Sour milk.	Milk, a little sour.
Cabbage, boiled, mashed, salted.	*Some vegetable that has been salted, but there is no other taste.*	Cabbage.	Turnips.
Orange juice, from fresh fruit.	*First sensation is of sweet, then of sour. Do not know what it is. Pleasant.*	Orange juice.	Juice of orange.
Yolk of hard-boiled egg.	*I do not know. It has no taste.*	Yolk of hard-boiled egg, told by texture entirely. What I called breakfast food before.	Like yolk of an egg.

TABLE C. SECTION IV. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. R.
Preserved quince, small piece.	*A very sweet fruit, but no distinct flavor.*	A piece of cooked quince.	It has the taste of quince.
Water, undistilled.	*Water.*	Distilled water.	Water.
Tincture of aloes with licorice.	*It is quinine, with something like licorice.*	A very bad taste, like bad medicine.	Dandelion extract.

TABLE C. SECTION V.

November 19, 1898.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.	MR. S.
Distilled water.	*Tepid water.*	Warm water.	Milk and water.	
Cabbage, boiled, the liquor only.	*Rather a sweetish taste, but do not know what it is.*	Cabbage flavor.	Know it, but can't name it.	Sour milk.
Beef broth, unsalted.	*A very unpleasant taste. Do not know what it is.*	Beef soup.	Beef extract and water.	Chicken broth.
Brandy, California.	*Vanilla.*	Whiskey.	Brandy.	Alcohol. It has an agreeable ethereal taste.
Distilled water, warm.	*Water.*	Water.	Milk and water, I think, not sure.	Water.
Pork broth, unsalted, (fresh pork).	*A flat washy taste, nothing decided.*	Beef flavor.	Beef soup.	Contains cabbage.
Navy beans, baked, thick gravy only.	*Oily taste. Do not know what it is like.*	Beans, baked.	Juice or gravy of baked beans.	Beans.
Malt extract.	*Slightly sharp taste at first, then a decided bitter, which persists.*	More like yeast than anything else.	Ale.	Yeast.
Baked sweet potato, scraped.	*Sweet potato.*	Sweet potato.	Sweet potato.	Sweet potato.

TABLE C. SECTION V. Continued.

SUBSTANCE.	*MRS. S.*	MRS. K.	MRS. L.	MR. S.
Oysters, raw, the liquor only	*Do not know whether this has any taste or not, former one so distinct.*	Oyster flavor.	Oyster juice.	Oyster soup.
Red raspberry juice, sweet.	*Good, sweet grape juice.*	Raspberry, slightly mildewed.	Raspberry juice.	Juice of red raspberry.
Mutton broth, unsalted.	*Tastes flat, as if it might be good if seasoned.*	Beef, I think something richer but can't tell.	Mutton broth.	Broth of meat, and tastes like cabbage.
Veal broth, unsalted.	*Tastes flat, like those others.*	Beef flavor.	Beef or mutton broth, much diluted.	Chicken broth.
Oyster stew, the liquor only	*Oyster soup.*	Oyster flavor.	Liquid from raw oysters.	Oyster soup.
Pork broth, salted, (fresh pork).	*Beef tea.*	Beef extract.	Beef extract, weak.	Chicken broth.
Distilled water.	*Water.*	Water.	Water.	Water.
Veal broth, salted.	*Stronger beef tea.*	Salted beef extract.	Oyster soup, or a mixture of milk and beef extract.	Chicken broth tinge of cabbage.
Oyster stew, salted, the liquor only.	*Just like oyster soup.*	Oyster flavor.	Like the liquid of oysters, mixed with something unpleasant.	Carrot.
Beef broth, salted.	*Beef tea.*	Salt and beef extract.	Had this several times; not sure; meat broth.	Chicken broth.
Mutton broth, salted.	*Still stronger beef tea.*	Salty mixture, beef extract.	Meat broth, same as last.	Like chicken broth.
Roast duck, chopped fine.	*Beef, cooked.*	Small bits of chicken meat.	Duck.	Chopped chicken.
Boiled mutton, chopped fine.	*Veal.*	Mutton in small pieces.	Meat, but insipid as if soaked in water.	Chopped chicken, I believe.
Fresh pork, boiled, chopped fine.	*Pork.*	Beef.	Boiled beef, I believe, not sure.	Chopped beef, possibly by texture.

TABLE C. SECTION V. Continued.

SUBSTANCE.	MRS. S.	MRS. K.	MRS. L.	MR. S.
Boiled beef, chopped fine.	*By texture I know it to be a meat of some kind. Should think it very young beef, or veal.*	Beef.	Boiled beef.	Beef; these tastes are alike to me.
Boiled veal, chopped fine.	*Beef.*	Beef.	Same as last two, boiled beef.	Beef, by texture.

TABLE C. SECTION VI.

November 25, 1898.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. O.
Distilled water, salted, warm.	*No taste, but it is not water.*	Like water in which matches have been soaked	Not much taste, slightly salty.
Raw potato, chopped fine.	*I do not know; seems like cabbage but is too fine grained.*	Raw potatoes; I think some dust in it.	Something like acorns.
Boiled pumpkin, strained.	*It has very little taste, but seems a little like squash.*	Pumpkin.	Something sweet, also flat.
Fresh pear, chopped fine.	*Pears, both by taste and the small granules.*	Pear, raw.	Sweet berry, a little fermented.
Liebig's extract of beef.	*Chicken broth.*	Meat broth, can't tell what.	Bouillon, a little salty.
Distilled water, warm.	*Warm water.*	Lukewarm water.	Least bit sour.
Roast pork, chopped fine.	*Turkey.*	Turkey, I think, but there was a fragment of bone, so am not quite so sure.	Boiled beef, salted, well-boiled with taste nearly gone.
Raw turnip, chopped fine.	*Cabbage, raw.*	Cabbage, raw.	Raw cabbage-heart, a little sweet.

TABLE C. SECTION VI. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. O.
Raw apple, chopped fine.	*Raw apple, both by taste and texture.*	Raw apple.	Grape juice, a little fermented. Don't recognize the other ingredients.
Roast turkey, dark meat. (There was onion in the dressing).	*It is meat, but has no distinguishing taste.*	Turkey, slight taste of onions; think the other may have been pork, (referring to roast pork, see above).	Boiled beef, mixed with a little onion.
Distilled water, salted.	*Like slightly salted water, might be a very weak soup, salt is the only distinct taste.*	Milk and water, with a little butter and salt.	A little salt water.
Malt extract.	*A disagreeable, bitter taste, but do not know with what to compare it.*	Fermented cider, I think, but it may be ale or porter.	Bitter, cherry wine or phosphate.
Preserved strawberries, juice only.	*It has a pleasant fruit taste, but I do not know what it is.*	Preserved strawberry juice.	Strawberry preserves.
Distilled water, warm.	*Water.*	Water.	Water.
Cranberry sauce, juice only.	*Cranberries, entirely by taste.*	Cranberries.	Sweetened orange juice.
Turkey, light meat, chopped fine.	*Turkey. This taste seems quite distinct.*	I can't tell. Slight taste of onion or something like it. Meat.	Boiled veal, salted.
Cocoa in milk, (as usually prepared, unsweetened).	*It has a slight bitter taste, which resembles coffee without sugar.*	Chocolate or cocoa in water, but it doesn't taste right.	Chocolate or cocoa, sweetened.
Celery, chopped fine.	*I think we had it before [turnip?], and I rather think I judge more by the texture than by taste. Yet I think there is some.*	Celery.	This is something like the second article we had. [Raw potato]. Cannot tell what it is unless it is tasteless celery.

TABLE C. SECTION VI. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. O.
Horse-radish, chopped fine. (The chopping removes to some extent the sharp, biting effect).	*It has a pleasant sweet taste before chewing, which brings out a sharp stinging taste. Do not know what it is. Might be mild horse-radish*	Horse-radish.	I do not know; never tasted anything like it.
Raw onion, chopped.	*A bitter sharp taste Do not know what it is.*	Onion.	Onion and something bitter.

TABLE C. SECTION VII.

November 26 and December 2, 1898.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Venison, fried and chopped fine.	*It is meat, tastes like beef.*	Beef.	Beefsteak, chopped.
Veal, fried and chopped fine.	*It is meat, but do not know what kind.*	Meat. I can't tell what kind; richer than the other.	Some kind of meat, veal.
Roast turkey, chopped fine.	*Turkey.*	Turkey or chicken	Chopped, cold turkey.
Turnip, boiled and mashed.	*Pumpkin. Has no taste. Judge by texture.*	Turnip, I think, but the texture doesn't seem quite right.	Mashed turnip.
Distilled water, warm.	*It has no taste.*	Water.	Like water.
Peanuts, roasted, chopped fine.	*Peanut. The taste is quite distinct. Knew it was a nut by texture.*	Roasted peanuts.	Roasted peanuts.
Almonds, chopped fine.	*Hickory nuts, but taste is not so distinct as peanut.*	Almonds.	I think it is chestnuts.
English walnuts, chopped fine.	*English walnuts. The slight bitter taste is quite distinct.*	Hickory nuts.	Like hickory nuts.

TABLE C. SECTION VII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Yolk of raw egg.	*Melted butter.*	Egg.	Rather a sweetish taste, very much like yolk of an egg, uncooked.
Cabbage, raw, chopped fine.	*It certainly tastes like cabbage.*	Cabbage.	Cabbage.
Coffee, black.	*Coffee, entirely by taste which is bitter.*	Coffee.	Coffee.
Cheese, New York.	*Cheese.*	Cheese.	Cheese.
Vanilla extract, somewhat dilute.	*Tea, weak.*	Water with a very little vanilla flavoring.	Vanilla flavoring.
White of raw egg.	*Some oily fluid, might be oil from a cooked fowl.*	Egg,—yolk and white beaten together.	White part of egg, raw.
Boiled onion, liquid only, unsalted.	*An unpleasant sweetish taste. I do not know what it is.*	Don't know what it is. Slight taste of onion.	Sweet taste. It may be onion.
Coffee, with cream and sugar.	*Coffee, with both cream and sugar.*	Coffee, with milk and sugar.	Sweetened coffee.
Almond flavoring extract.	*It tastes like mountain tea, just a little.*	Vanilla. The time I said vanilla before I was wrong.	Almond flavoring.
Onion, boiled, with milk and salt.	*I rather think it is some vegetable cooked in cream, but it is not familiar to me; unpleasant, sweet taste.*	Boiled onion.	A sweet taste. I think cooked onion.
Tincture of ginger.	*Very sharp burning taste, like red pepper.*	Ginger, I think Jamaica.	Ginger and alcohol.
Venison, fried, small square piece.	*Beefsteak.*	Beefsteak.	Beef.
Veal, fried, small square piece.	*Veal. Texture is finer than the beef and taste seems more delicate.*	Boiled beef.	Porterhouse steak.

TABLE C. SECTION VII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Turkey, roasted, small square piece.	*Turkey. A part of the breast; latter I guessed because one side was perfectly smooth.*	Turkey.	Turkey or chicken.
Fresh butter, unsalted, partly melted.	*Some kind of oil, but it has no taste.*	Something oily, like thick cream.	Butter without salt.
Cornstarch, cooked with milk.	*Like cornstarch, cooked without any sugar.*	Like cornstarch, not well cooked.	Thick, like cornstarch.
Tapioca, cooked in water.	*Clear starch.*	Tapioca.	No taste. Thick.
Buttermilk, strained.	*Like cornstarch dissolved in water.*	Milk, slightly sour	Sour milk.
Boiled rice, mashed.	*Rice. A distinct starchy taste, but I am sure the texture assists some.*	Rice.	Ground rice.
Graham bread.	*I think it is bread, perhaps whole wheat.*	Bread, white, a little something sweet on it.	Brown bread, or whole wheat flour.
White bread.	*Bread, made from white flour.*	Whole wheat bread.	White bread.
Boston brown bread.	*Brown bread, Boston baked.*	Corn meal cake.	Brown bread, a little taste of graham.
Whole wheat bread.	*White bread.*	Bread, heavier than white, but not like whole wheat.	Some kind of bread.
Rye bread.	*Rye bread.*	Bread, finer texture; white, a little like salt-rising bread.	White flour bread.
White bread, spread with salted lard.	*White bread with butter.*	Bread with lard.	White bread, buttered.
Lemon juice.	*Citric acid.*	Lemon juice or citric acid.	Lemon juice.
Fresh butter, unsalted, hard.	*Some kind of fat, but it has no taste.*	Like thick cream; seems a little too oily.	Fresh butter.

TABLE C. SECTION VII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. R.
Tea, ordinary infusion.	*Water.*	Tea.	Tea.
Sodium carbonate, $1\frac{1}{4}\%$ solution.	*Somewhat familiar but not known. A little taste of soda. There is a slight alkaline effect on the flesh.*	Solution of soap. Lye.	Lime water.
Cherries, canned, juice only, rather sour.	*Some kind of fruit juice. Might be cherry.*	Cherry juice.	Cherry juice.
Coffee, ordinary decoction.	*Slightly bitter, but I do not know what it is.*	Coffee.	Coffee.
Currant jelly.	*Jelly; do not know what kind.*	Currant jelly.	Jelly, grape.
Maple syrup.	*Maple syrup.*	Maple syrup.	Maple syrup.
New Orleans molasses.	*New Orleans molasses.*	Molasses. I think not New Orleans but I don't know what to call it.	New Orleans molasses.
Sorghum.	*Sorghum.*	New Orleans molasses.	Molasses, I do not know what kind.
Syrup, "Honey Drop."	*I do not know. It tastes like soft butter scotch.*	Molasses, with a taste of peanuts like melted peanut candy.	Sugar syrup.
Honey.	*Honey.*	Honey.	Honey.

TABLE C. SECTION VIII.

December 3, 1898.

SUBSTANCE.	MRS. S.	MRS L.	MRS. K.
Mace. (This and the following were all pure ground spices).	*Nutmeg.*	Nutmeg.	Nutmeg.
Allspice.	*Some spice, may possibly be allspice.*	Cloves.	Cloves.
Cinnamon.	*Cinnamon.*	Cinnamon.	Cinnamon.

TABLE C. SECTION VIII. Continued.

SUBSTANCE.	MRS. S.	MRS. L.	MRS. K.
Cloves.	*Cloves.*	Cloves, the other was allspice.	Cloves. Allow me to change other to allspice.
Ginger.	*Certainly is pepper.*	Ginger.	Ginger.
Mustard.	*Seems something like mustard, but not strong enough*	Mustard.	Mustard.
Black pepper.	*Red pepper.*	Black pepper.	Pepper.

The following additional substances were tried at a later date and with different control observers for the purpose of determining the effect of heating the substances and so, by reducing them to a like consistency, eliminating to some extent the sense of touch. Only indifferent success attended this effort for the reason mentioned above, p. 99. The syrups and molasses were heated to a temperature of 80°C. but before they could be tasted from a spoon had fallen to about 60°.

TABLE C. SECTION IX.
April 11, 1899.

SUBSTANCE.	MRS. S.	MISS. W.	MR. R.
Sorghum.	*Sorghum molasses.*	Hot molasses, sugar cane; recognized by smell before tasting.	A peculiar, familiar sweet taste. Not a perfect maple taste.
New Orleans molasses.	*New Orleans molasses.*	Molasses with something else added. Can't tell what.	Sorghum.
Sorghum.	*New Orleans.*	Just simple molasses, or sorghum.	A syrup resembling the sugar beet. Less sharp.
Syrup, "Honey Drop."	*Syrup. Do not know what kind. Peculiar velvety texture.*	Sorghum and syrup.	Seems like honey.
Maple syrup.	*Cane sugar syrup.*	Maple syrup.	Maple syrup.

TABLE C. SECTION IX. Continued.

SUBSTANCE.	MRS. S.	MISS W.	MR. R.
Light brown sugar, melted.	*It is syrup with a slight maple taste, but not strong.*	Syrup, made from brown sugar.	Resembles New Orleans syrup.
Dark brown sugar, melted.	*It is maple, but not strong.*	Syrup, made from brown sugar.	Syrup. Like New Orleans. Possibly trace of maple.
Maple syrup.	*Maple syrup.*	Maple syrup.	It contains maple syrup.
Honey, strained.	*Honey. Recognized by taste and smooth texture.*	Syrup made from granulated sugar.	Honey, sure. Possibly I was wrong before.
Maple sugar, melted.	*Maple syrup.*	Melted maple sugar.	Maple syrup.
Brown sugar, dried and pulverized.	*Brown sugar.*	Sugar, known as C. sugar.	The soft brown sugar.
White sugar, pulverized.	*Cane sugar, both by taste and texture.*	Granulated and powdered sugar mixed.	Soft brown sugar.
Maple sugar, dried and pulverized.	*Maple sugar, by the taste.*	Powdered maple sugar.	Maple sugar.

In discussing the results of the experiments as shown in the tables, we might say that in theory those substances which are not recognized by any of the observers depend for their recognition upon sensations of sight; that those substances recognized by the normal observers, but not by the anosmic, depend upon sensations of smell for their recognition, while those recognized by all the observers depend upon either taste, touch, or muscle sensations. We might indeed go one step farther and say, since Mrs. S. did not possess any superiority in the sense of taste, that those substances recognized by her and not by the other observers, depend for their recognition upon touch or muscle sensations. It is obvious, however, that experiments of this kind cannot have that degree of exactness which would justify such complete generalizations. Sugges-

tion plays an important part in taste and a few of the correct judgments were evidently the results of happy guessing. Making due allowances for these facts, the following lists have a considerable value as indicative of the part played by the several kinds of sensations in taste perceptions. The most interesting list, though perhaps not the most instructive, comprises those substances recognized by the two normal observers, but not by the anosmic. The characteristic "taste" of these substances is presumably their odor. They are as follows, omitting those made doubtful by giving different results upon different trials: Tincture vanilla, vanilla extract, spirits of almond, pineapple syrup, orange, lemon, banana, grape, quince, strawberry, fig, tea, cocoa, chocolate, milk, sour milk, vinegar, claret, oil of rose, rhubarb, onion, boiled turnip, navy beans, (liquid form,) liquor of raw oysters, yolk of egg, white of egg, and kerosene. This list, however, should be supplemented by the second list including those substances recognized by one of the normal observers but not by the anosmic. No doubt, concerning these the same conclusion should be drawn. They are as follows: Peach syrup, currant jelly, wintergreen, port wine, sherry wine, brandy, unsalted butter, cream, olive oil, vaseline, cabbage, pumpkin, raw potato, beef broth, mutton, and mutton broth. These lists, it should be observed, do not include all the substances, even of those tried in my experiments whose characteristic "taste" is really an odor, for some things, such for instance as pear, apple, cheese, and the different kinds of syrups and molasses have also a sufficiently characteristic texture or consistency to be recognized by the anosmic observer. Raspberry and cherry and the fruits in general should be included in these lists, although upon a single occasion these two were correctly named by the anosmic observer. Raspberry juice is sweet and moderately sour and slightly viscous and it is not strange that it should be correctly named once out of three trials without its characteristic odor.

The importance of sight sensations in taste is seen in the list of substances which, while usually recognized in daily experience, failed of recognition either by one or both of the

normal observers when blindfolded. Most conspicuous among these are the various kinds of meats and breads. They have no taste except that of the salt with which they are prepared or possibly a very faint sweet, while their characteristic odors are too faint to serve as marks of recognition. It should be noticed that the unsalted meat broths could not be recognized even as meat broths by the anosmic observer, much less distinguished, but as soon as they were presented with salt they were recognized as meats but not differentiated. There was a tendency on the part of all the observers to call all the meat broths, beef or chicken, where the effect of suggestion is apparent. With the chopped meats, or even with the larger pieces, the case is not much better. Chicken, turkey, and pork have in common a characteristic tenderness which assists in their identification by the muscle sense, but they are not easily distinguished from each other. It is not likely that the turkey would have been named at all by any of the observers in the experiments except that it was presented immediately after Thanksgiving, and furthermore smelled unfortunately of the onion in the dressing. The anosmic observer recognized it on two occasions and in one case explained that the recognition was due to the fact that the piece was very smooth on one side and was inferred to be a portion of the breast. The anosmic observer succeeded better than any of the others in identifying the different kinds of bread, a fact due to their differences of texture. One of the observers correctly named roast duck, but she had eaten this the day before. Among other things in the "taste" of which sight sensations have an important part, are butter, cream, olive oil, and various fruits and vegetables. It is noteworthy that Mrs. O., a housekeeper of long experience, when blindfolded failed to recognize raw turnip, raw potato, boiled pumpkin, cranberry sauce and fresh pear. If turkey, for instance, has no taste and very little or no characteristic odor, it may be asked why it is that it is so much prized. This is a problem for some student of the psychology of the emotions to work out. But of course turkey does have a "taste." Its "taste" is made up of a nicely

adjusted set of relations between a number of sensations, of which the most important are muscle, touch, and sight sensations, with a faint odor and perhaps a faint sweet taste and the indispensable taste of the accompanying salt. If it is asked why turkey is so commonly considered to be better than chicken, we must perhaps be satisfied with the prosaic answer,—because there is more of it. If it is still further asked, why, then, is quail better than either turkey or chicken, we may perhaps answer,—because there is less of it, that is, it is rare and hard to get. Deep rooted associations connected with hunting and the chase may have much to do with our appreciation of certain kinds of meat, and to appreciate them fully we must *see* them, or at least be told what they are.

If now we consider the results of the experiments in their relation to the problem of taste sensations themselves, we are met by the difficulty that the experiments afford no means of separating sensations of taste from those of touch and the muscle sense. We may however inquire whether the results tend to weaken or confirm the hypothesis which we have made above, namely, that there are only four taste sensations, that these play a very unimportant part in the discrimination of foods and drinks, and that they are not fused or united to form the taste perceptions of common experience. It has already appeared that sight sensations and particularly smell sensations make up an important part of our taste perceptions, that they are indeed the essential elements in the "taste" of our most common foods and drinks—such as fruit, meats, bread, butter, tea, coffee, cocoa, wines, etc., which are among the things that *seem* to have the most decided and characteristic taste. It is at least conceivable that the remaining substances which can be recognized without the aid of sight or smell, may be recognized by the delicate sense of touch possessed by the tongue, and by the muscles used in chewing, together of course with the help which may be given by taste sensations of salt, sweet, sour, and bitter. About forty substances altogether were correctly named by Mrs. S. As we read this list we notice that a large percentage of the substances, if not all of

them, have a characteristic texture, or consistency, or else a characteristic biting or astringent effect or pungency. With a few exceptions they are the substances which could not be presented in liquid form. The list is as follows: Camphor, peppermint, cinnamon, allspice, pepper, mustard, cloves, mace, oatmeal, potato, beans, squash, raw apple, baked apple, lard, date, cheese, sweet potato, oyster stew, celery, peanuts, cabbage, cornstarch, rice, cherry juice, grape juice, licorice, honey, tomato, chicken broth, white of boiled egg, beef broth, pork, turkey, pear, cranberry, horse radish, English walnuts, white bread, brown bread, rye bread, coffee with cream and sugar, maple syrup, sorghum, New Orleans molasses. Some of the substances mentioned in this list were tried more than once, and with other trials failed of recognition. Maple syrup was correctly named three times but failed of recognition once, while melted brown sugar was called maple twice. A few of the judgments were no doubt good guesses, especially perhaps those of the sweet and sour substances, like the few fruits that were correctly named. The majority, however, were probably recognized by the muscle and touch sensations. In many cases, indeed, this was admitted by the observer to be the means of recognition. Cheese could be easily recognized without taste, smell, or sight by its biting effect together with its characteristic consistency. Pear is sweet, sour, and granular; cranberry, sweet, sour, and astringent. The chopped nuts would be revealed by their texture and the English walnuts differentiated by their oiliness. The texture of horseradish together with its pungency would be its sufficient marks. A mildly bitter limpid fluid with a slight sweetness and an oily feeling would probably be coffee with cream and sugar. Oatmeal, potato, squash, lard, rice, cornstarch, etc., all have their characteristic textures. The syrups and molasses could be recognized by their sweetness and differentiated by their different consistencies. Some of the answers which Mrs. S. gave when she failed to name the substances are instructive in their bearing upon this subject. Orange juice (See Section IV) was said to taste first sweet and then

sour. Preserved strawberries (See Section VI) were said to have "a pleasant fruit taste," the marks of the "fruit taste" no doubt being a slight viscosity accompanied by sweet and sour, the predominance of the sweet making it "pleasant."

On the whole the experiments confirm, so far as they go, the hypothesis made in this article, and, while not diminishing the importance which has been given to sensations of smell in the "tastes" of common experience, they indicate that touch and muscle sensations play an unexpectedly important part.

A special supplementary series of tests was made upon the taste of coffee and tea. These are not reported in detail because they merely confirm what is already well known, though not always clearly comprehended, about these tastes. Strong and weak decoctions of Mocha and of Java coffee and strong and weak infusions of Gunpowder and of Oolong tea were tried with normal observers blindfolded and with closed nose. Distilled water of the same temperature was used as a control substance. The same preparations together with very dilute solutions of sulphate of quinine were tried with the anosmic observer. Tea and coffee in finely powdered form were also used in the tests. Briefly stated the conclusions are that coffee and tea have a bitter taste, not to be distinguished from the bitter of quinine or any other bitter substance when the intensity is the same. Weak coffee and tea are, without the sense of smell, often confused with water of the same temperature, and if strong are confused with bitter substances. Apart from their odor, tea and coffee with cream and sugar can scarcely, if at all, be distinguished from each other, but if clear and of medium strength they may be distinguished by the slight astringency possessed by the tea.[1] The latter

[1]One of the experiments which I tried with tea and coffee with Mrs. S. may be mentioned. I prepared ten teacups and into each I put a tablespoonful of rich cream and a teaspoonful of sugar. The cups were then filled with the following preparations and kept during the tests at a temperature of 55°C. which is about the temperature of coffee as it is drunk at the breakfast table. No. 1 was filled with water; No. 2, with a solution of sulphate of quinine in the proportion of 1 part quinine to 20,000 parts water; No. 3, with sulphate of quinine, 1 to 10,000; No. 4, with coffee

is of course due to the excess of tannin possessed by the tea, while the bitter taste of tea and coffee is due to the alkaloids, theine and caffeine, and the characteristic aroma to the volatile oils which they contain. To the bitter theine and caffeine belong the peculiar dietetic and stimulating effects of tea and coffee.

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as ordinarily prepared, using 1 tablespoonful of ground coffee to 1 cup of water; No. 5, with tea, ¼ teaspoonful black tea to 1 cup of water; No. 6, with quinine, 1 to 5,000; No. 7, with coffee, ½ tablespoonful to 1 cup of water; No. 8, with tea, ½ teaspoonful to 1 cup water; No. 9, with tea, 1 spoonful to 1 cup of water, but without sugar; No. 10, same as No. 9, but with the cream and sugar. Mrs. S. was then blindfolded and the cups were brought to her one at a time and she tasted each mixture directly from the cup, taking whatever amount she wished, rinsing the mouth with lukewarm water after each test and writing her judgments upon prepared slips. The judgments were as follows: No. 1, (water): "Tastes like weak tea with cream and sugar" No. 2, (quinine): "Coffee." No. 3, (quinine, stronger): "Tastes bitter, like poor coffee." No. 4, (coffee): "Coffee with some cream and sugar." No. 5, (tea): "It has a sweet creamy taste; do not know what it is; do not like it." No. 6, (quinine, stronger): "Very bitter; do not think it is coffee; might be a grain drink." No. 7, (coffee, weaker): "Hot water, with cream and sugar." No. 8, tea (stronger): "I do not know what it is; tastes sweet." No. 2, (quinine, repeated): "Coffee." "I think it is weak but of good grade." (The latter in answer to an inquiry about the quality of the "coffee.") No. 5, (tea, repeated): "Milk." No. 9, (tea, strong, with cream only): "Coffee, with cream; do not quite like the flavor." No. 10, (tea, strong, with cream and sugar): "I think it is coffee with sugar. I do not know whether it is good grade or not." It may be added that the observer is a habitual user of coffee of which she is fond, and which she takes with cream and sugar. She does not use tea. Quinine is a drug which she says is peculiarly distasteful to her.

Some Peculiarities of the Secondary Personality.[1]

BY
PROFESSOR G. T. W. PATRICK.

Of the many unsolved problems in psychology, that of automatism is perhaps the most baffling. Automatic utterances, whether in the form of writing or the speech of the so-called trance-medium, present certain peculiarities which distinguish them so clearly from the utterances of normal subjects as to require some special explanation. Other abnormal mental conditions, such as mania, melancholia, hypnosis, or hallucinations, present peculiarities each of its own kind, but these are by no means so puzzling as those of automatism. If not at present fully explained, we believe that they may be eventually understood as exaggerations or perversions of normal forms of mental life. In automatism, however, we are apparently confronted with phenomena of a different kind. They belong to that class which the scientist of the day would call 'remarkable,' demanding instant attention and careful verification, and requiring if they persist some special explanation. Indeed the extremely striking character of some of the phenomena of automatism may be illustrated by the nature of the hypotheses that have been made to explain them. I have in mind, in particular, one series of automatic utterances which have been under investigation for nearly fourteen years by psychologists trained in scientific methods, and at the end of this time one of these psychologists, who has been most intimately connected with the investigation,

[1] Reprinted from The Psychological Review, Vol. V., No. 6, November, 1898.

reckoned to be a man of sanity and careful logical habits, has proposed, as the only hypothesis capable of explaining the facts, that the person from whom the utterances come is 'controlled' by one or more disembodied 'spirits' of the deceased.[1]

Such a hypothesis violates almost all the conditions to which a legitimate hypothesis should conform. It does not connect the phenomena in question with any other known facts or laws. Proposing as the basis of explanation certain wholly unknown forms of being, it admits of no deductive inference of consequences. It cannot, furthermore, be clearly and definitely conceived, and does not, finally, explain all the facts. I mention this merely to illustrate the straits which psychologists are in to explain the phenomena of automatism. The peculiarity of the situation is not greatly lessened when we learn that other psychologists maintain in all seriousness that, without recourse to the 'spirit' hypothesis, the phenomena may all be explained by 'telepathy'—a doctrine itself of questionable antecedents.

Under these circumstances, what should be the attitude of psychologists towards automatism? No one can doubt the answer which every scientist would make to this question. We want more facts and more hypotheses—especially facts. While this is the true attitude, unfortunately it is not the one which has usually been held. Too many have treated the whole subject with neglect if not with actual contempt. This wholly unscientific attitude has been the result of no real want of the spirit of investigation on the part of psychologists; it has been due rather to the frowns of the science world in general, and this again is explained if not excused by the unhappy history of the phenomena of automatism. These bear, at least in England and America, the corrupting marks of evil associations. They suggest all sorts of charlatanry and superstition. It has been felt that to maintain the dignity of experimental psychology, this subject and certain

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[1] RICHARD HODGSON, LL.D. *A Further Record of the Observations of Certain Phenomena of Trance.* Proceedings of the Society for Psychical Research, February, 1898.

related ones must be ignored. They have been almost uniformly kept out of American psychological laboratories, where infinite labor has been spent upon other probably less fruitful problems. But experimental psychology has now long passed its probationary period and may quite freely choose its subjects for research, and at present there is perhaps no other subject promising to throw more light upon certain dark chapters in mental science than that of automatism. No one can read the reviews that have appeared of Dr. Hodgson's report upon the trance-utterances above mentioned, indicating the self-confessed confusion of those most intimate with the case, coupled with a half readiness to accept almost meaningless explanations, without feeling the urgent need of a wider acquaintance with related facts. To be sure, the psychical research societies and a number of distinguished and devoted French investigators have been for some years assiduously cultivating this field. Whether we judge the results of their labors, however, by the conclusion lately arrived at by Dr. Hodgson, or by the excellent summary contained in Mr. Podmore's recent Studies in Psychical Research, the conviction is strengthened anew that we want more facts and more explanations. Without denying its great debt to psychical research, experimental psychology may profitably take up the problem of automatism, apply still more rigorous methods, and, what is of greater importance, include in its investigations a larger number of more simple cases. In the psychical research literature, one is wearied by the perpetual recurrence of a few remarkable 'classical' cases. It would be desirable to have fresh cases, a good many of them, and they should be simple ones. The cases reported hitherto have been too complex and remarkable, or rather the examination has not included a sufficient number of the less complex and less remarkable. The primary object of the present paper is to urge the extension of experimental work in this direction. The thorough study of simple cases of automatic writing and of all forms of automatism in normal healthy subjects is wholly practicable in the laboratory and certainly desirable.

Encouraging beginnings have been made in American laboratories by Solomons and Stein[1] in two researches upon normal motor automatisms, and by Jastrow[2] and Tucker[3] in researches upon involuntary movements.

About three years ago I undertook, as a contribution to this subject, to make a study of a simple case of automatic writing. Owing to the absence of the 'subject' from the city for two years, the study was only recently completed. I present it now rather as an indirect means of furthering my object above mentioned than as a study possessing any intrinsic value in itself. For this reason I add certain details of procedure, which, while familiar to every 'psychic researcher,' may perhaps be useful to the larger body of investigators which I conceive to be demanded by the importance of the problem. I wish also to use the occasion to call attention to certain peculiarities of the secondary personality appearing in this and in other cases, and incidentally to notice their relation to certain hypotheses that have been made to explain them. I shall, therefore, rather freely preface the account itself with some general remarks and some mention of other experiments that I have made. I use the term 'secondary personality' advisedly, finding it preferable to secondary consciousness, or subliminal or subconscious personality, or any other phrase, as it is justified by the facts, and is in harmony with any, even the 'spirit,' hypothesis. In automatic writing, for instance, we find ourselves in communication with a source of intelligence that hears and answers questions, reasons, exhibits pleasure and anger, assumes a name which it retains from day to day and from year to year, and displays an accurate memory extending over long intervals of time. To such a source of intelligence we cannot refuse the name of personality. When in connection with the same physical organism we find a synchronous or alternating intelligence, exhibiting different mental peculiarities, having a different

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[1] Psychological Review, Vol. III., p. 492. Ibid., May, 1898.

[2] American Journal of Psychology, Vol IV., p. 398.

[3] American Journal of Psychology, Vol. VIII., p. 394.

name and displaying a different set of memories, we find it not only convenient but suitable to speak of a primary and a secondary personality. This secondary personality may be an apperceptive unity corresponding to a special grouping of association tracts in the subject's brain, it may be some lower mental stratum belonging to a sort of universalized psychic faculty, or it may be the 'spirit' of my deceased grandfather; it may or it may not be subliminal; it is even conceivable that it should not be conscious, but it bears all the common marks of personality.

Thus far the problem presents no very serious difficulty. The mere fact that there should be in connection with the same organism two personalities is not more wonderful than that there should be one. There is nothing in our present knowledge of the ego either from the psychological or physiological standpoint preventing us from admitting that the elements which usually join in a single group may, under certain conditions, so associate themselves as to form two or three or any number of different groups, nor indeed that the same elements, as for instance memory images, may at once form a part of both or of several systems. Furthermore, there is another circumstance which would seem to make the scientific study of the secondary personality at least possible. It has certain pretty clearly defined marks, traits, or peculiarities, capable of logical description. The presence of these traits in all the cases of automatism which have been reported forces upon us the conviction that they all belong to the same general class and that the investigation of the simpler cases may throw much light upon the more complex ones. If we compare a simple case of automatic writing, such as may be found in one of almost any company of schoolgirls, with the wonderful case reported by Dr. Hodgson, the difference is as great as between a kitten and a tiger, but perhaps not greater, for a careful observer will discover 'marks' which indisputably place them in the same genus. What we need now is a more complete description of these marks. Besides the case presented below, I have recently had opportunity of studying two other

cases of automatism, both instructive, neither of them very remarkable, and in all of them I have been impressed by the presence of the usual marks, for instance, suggestibility, fluency, absence of reasoning power, exalted or heightened memory, exalted power of constructive imagination, a tendency to vulgarity or mild profanity, the profession of 'spirit' identity and of supernatural knowledge, and finally a certain faculty of lucky or supernormal perception difficult to name without committing oneself to a theory, which therefore we may call a kind of brilliant intuition. It seems to me not impossible ultimately to make a complete list of these marks, and then perhaps to explain why they are characteristic of the secondary personality. Some time ago I paid a visit to a 'medium' residing in a small western city. She is a married woman with a family, and was made known to me by one of my students whose family was intimately acquainted with the woman, having known her from her girlhood. My investigation left no doubt in my mind that she is an honest woman and passes into a genuine trance, and upon awaking is ignorant of her trance-utterances. These take the form of the personality of a Quaker doctor or of a little girl named Emma, both professing themselves to be 'spirits' of deceased persons, and to have supernormal and supernatural knowledge. I conversed for an hour with 'Emma,' and was throughout struck by the remarkable likeness in the general form of the utterances to those more remarkable ones recorded by Dr. Hodgson and others, so that I cannot doubt that we have to do with phenomena of the same genus and species, and that the explanation of the simpler case, were it at hand, would throw much light upon the more complex one. The similarity extended even to an accurate and astonishing statement, made (as so often happens) at the very beginning of the sitting, about my place of residence and my occupation. This was certainly an interesting trait and in need of explanation, although it would not have suggested to me the hypothesis that 'Emma' was the 'spirit' of a real person, for, however difficult it might be for a woman who had apparently never seen or heard of me before

to tell me my home and occupation, it would evidently be more difficult for a young girl to do so who had lived and died prior to the circumstances and relations mentioned. If we have to ascribe to our communicator powers of perception transcending time and space, it makes our hypothesis needlessly complex to ascribe them first to a 'spirit' and then locate the 'spirit' in the person before us. If we ascribe them directly to this person we avoid the trifling inconvenience of supposing that things are known before they happen, or, if we must violate time and space, we have to violate less of both.

Again, not long ago, I became acquainted with a young girl who was an automatic writer and whom I had several opportunities of studying. She wrote rapidly and legibly, only requiring that some other girl should hold the pencil with her. I convinced myself that the writing was purely automatic. It usually purported to come, and was sincerely believed by the girl to come, from the 'spirit' of her deceased mother. I shall mention one or two characteristic utterances from this case, but what I wish to emphasize is merely that the general form of the utterances was so similar to the others which I have studied and to those referred to above, that I cannot doubt that we have here again to do with closely related if not identical phenomena, and that the full explanation of the one would remove the mystery from the other. In all the writing which I saw from this subject (I shall mention some other examples from it below), there was one utterance and one only of the brilliantly intuitive type, and this again came early in the first sitting. In response to my questions, the correct answer was received that I had three sisters and two brothers, that the brothers were both younger and one of the sisters younger and two older. In response to my inquiry about their names, one of my sisters' names, a common one, was given, and then 'Gussie' was written which was spontaneously changed to 'Bessie,' the latter being correct. Admitting that the chances of correctly guessing such a combination as the above at the first guess are too small to make this a probable explanation, and admitting that the young girl, who

was an entire stranger to me at the time, could not have known in any normal way what the most intimate friend that I had in the city could hardly have known, what is the most that can be made out of such an utterance? If found to be a real intuitive utterance, not conforming to the usual laws of perception, memory, or constructive imagination, and if found to be similar to a sufficient number of other automatic utterances, it becomes an interesting mark of the secondary personality, but so far as I can see is not consistent with one more than another of the various hypotheses that have been offered. Probably no thoughtful investigator would apply the 'spirit' hypothesis, for instance, here, but so vitiated have we become in our logical methods when we enter the field of psychical research, that it seems to be generally accepted that if we could adopt this hypothesis it would explain utterances of this class. But, however difficult it might be to understand how the young girl could have known about my family, it would be still more difficult to believe that her deceased mother, who had never even heard of me, could have known, and there was no time to ascertain by inquiries. It is easy for the popular mind to understand all sorts of telepathic, clairvoyant, and time-obliterating powers when attributed to 'spirits' instead of every-day people, and the history of philosophy, despite the warnings of William of Occam, is full of that kind of reasoning. It has become very rare, however, in modern science. The 'spirit' hypothesis accounts for these peculiar phenomena of automatism in the same way that Descartes' 'animal spirits' accounted for the interaction of mind and body, or that the mythological tortoise explained the supporting of the world. From the logical point of view, however, it seems to me that little better can be said of Mr. Myers' theory of a 'spectrum of consciousness indefinitely extended at both ends,' with its 'telepathic and clairvoyant impressions,' 'falling under some system of laws of which supraliminal experience could give us no information' and 'transcending in some sense the limitations of time as well as of space,' having powers 'subject, not to the laws of the known molecular world, but to laws of that

unknown world in which the specific powers of the subliminal self are assumed to operate.' This is a metaphysical, not a psychological hypothesis.

The subject of the experiments which I wish to mention in more detail is a young man, 22 years of age at the time the experiments began. I shall speak of him in the following account as Henry W. He is now a graduate of the University of Iowa, a young man of unquestioned integrity, a quiet and intelligent student, standing high in his class and respected by all who know him. His parents are honest farming people, both native Americans. He has never exhibited any signs of abnormality of any kind, excepting the automatism to be described. He has good physical health and mental balance. Neither he nor his parents are spiritists. He has an aunt, however, who is a spiritist, and about four years before these experiments were begun he had some conversation with her upon the subject and probably opened some books relating to it. This, however, he says, made no impression upon him, and if he casually heard or read at that time any spiritistic phrases, such as 'pass out' for 'die,' he has no conscious recollection of them. He has no interest in the subject and has regarded it, so far as it has entered his thoughts at all, as a curious superstition. About the time of the beginning of the experiments, he became interested in hypnotism, and attended two or three times the performances of a travelling hypnotist, offered himself as a 'subject,' and proved to be an excellent one. He had never previously been hypnotized.

Shortly after this, having read of post-hypnotic suggestion, he inquired of me about it, and at his request I made a trial of it with him. Hypnosis was readily induced by a few suggestions, and I told him that exactly five minutes after he awoke he would go to the next room, secure a book from a desk and bring it to me. A few other simple tests were made, which, though commonplace in themselves, should be mentioned here for reference later. Hallucinations, both positive and negative, were readily induced. I suggested that a small barbed-wire fence was stretched across the floor, over which it would

be necessary for him to step carefully. This hallucinatory fence he saw and stepped over with great care. Upon awaking he remembered nothing of what he had heard or done. Exactly five minutes after awaking he carried out in detail the suggestion about the book. A few days after this, the subject of automatic writing having come to his attention, Henry W. incidentally mentioned to me that when he held a pencil idly in his hand, his hand moved continuously, making scrawls but never writing anything. I therefore made an appointment with him for the study of automatic writing. Three sittings were held and then a period of two years intervened. Then followed three more sittings. All were held on Saturday mornings. The procedure at each morning's sitting was as follows: I provided a quiet room and one assistant. At the second sitting only, others were present. A plentiful supply of very large sheets of smooth brown paper was provided. The subject was so seated with his right side toward the table, that his body was slightly turned away. His right hand held an ordinary pencil in an easy position on the paper.[1] His head was turned slightly to the left, and he held in his left hand an interesting story-book or sometimes the morning paper, which he read and to which he was instructed to give his whole attention. No screen was used, as the sub-

[1] I have never found the ordinary planchette of any use in automatic writing. When it is discovered that two persons succeed better in writing than one, both may grasp a common lead pencil, one hand above the other. The instrument used by Professor Jastrow, consisting of a glass plate upon glass-marble rollers, whether used for automatic writing or any involuntary movements, has the disadvantage of moving by its own momentum when once started. When it is necessary to 'educate' from the beginning an automatic writer, a delicate planchette mentioned by Miss Stein may be used. It consists merely of a board swung from the ceiling by a small wire. The one used in our laboratory consists of a light board six inches square, upon which the fingers rest as upon the common planchette. Through the board is a hole fitted with a glass tube in which a pencil is placed so that it will move up and down. A weight attached to the top of the pencil keeps it pressing lightly and evenly upon the paper below. Such a planchette, swung from the ceiling over the table, will glide around upon a large sheet of paper with the slightest effort, the pencil point always leaving its tracings.

ject could not see the writing without turning his head. The sittings lasted two or three hours with intervals of rest. The writing was usually quite clear, but occasionally illegible. If illegible, the communicator was asked to write the answer again. At one time I suggested to the communicator that he was a good penman, his chirography being round, clear and rapid. Instantly it became so and gave us no more trouble at the time. Henry W. never knew what he had written without reading it, except in a few instances when, his attention being allowed to wander from his book or newspaper, by following the movements of his hand he could tell something of the communication. He was much interested in the writing and was occasionally allowed to look at it. When it was nearly illegible he was never able to decipher it better than the others. The questions were either prepared beforehand and numbered or else taken down and numbered by the assistant, who also numbered the answers as written. My space will not permit me to give more than a portion of the questions and answers, nor would it be profitable to do so. They may be classed in three groups: Those of the first group were intended to bring out all the information possible about the communicator himself, his past history, his present mode of existence, his mental habits and his emotional peculiarities. The second group was intended to test his professed supernormal knowledge. The third group was directed to possible remarkable powers, such as telepathic knowledge, mathematical ability, hypermnesia and prophecy. The questions of the first group were connected more directly with the object of my inquiry. No remarkable telepathic or intuitive powers were discovered. If such powers had been found, they would have been of interest, but hardly more important for gaining a thorough knowledge of the secondary personality than more simple if less striking traits.

The first sitting opened as follows:

Q. Who are you?
A. Laton.

This was illegible, and Henry W. was allowed to look at

the writing. He read it as 'Satan' and laughed. A further series of questions revealed the name as 'Laton.'

Q. What is your first name?
A. Bart.
Q. What is your business?
A. Teacher.
Q. Are you man or woman?
A. Woman

No explanation of this answer was found. Laton assumed throughout the character of a man.

Q. Are you alive or dead?
A. Dead.
Q. Where did you live?
A. Illinois.
Q. In what town?
A. Chicago.
Q. When did you die?
A. 1883.

Then followed many questions, first relating to the bill of fare of Henry W.'s dinners for one, two, and three weeks back. Laton could give the *menu* somewhat correctly for two weeks back, but beyond that he said "I don't know." His memory of them seemed somewhat but not greatly superior to Henry W.'s. Various problems in mental arithmetic were given, the simplest being 16 x 9. The answers were always promptly written and were uniformly wrong. Tested upon the dates of well-known historical events, his answers were all incorrect. Asked about my mother's name he wrote 'Mary Peters,' but afterward changed it to 'Lucy Williams,' both wholly wrong. My sisters' names were given as 'Winnifred,' 'Jennie,' and 'Carrie'—all wrong.

Q. Have you supernatural knowledge, or do you just guess?
A. Sometimes guess, but often spirit knows. Sometimes he will lie.

The next sitting was held two days later.

Q. Who is writing?
A. Bart Laton.
Q. Who was mayor of Chicago when you died?
A. Harrison. [Carter Harrison was mayor of Chicago from 1879 to 1887.]
Q. How long did you live in Chicago?
A. Twenty years.

Q. You must be well acquainted with the city.
A. Yes.
Q. Begin with Michigan avenue and name the streets west.
A. Michigan, Wabash, State, Clark (hesitates)—forget

Henry W. is then asked to name the streets, and can name only Michigan, Clark and State.

Q. Now your name is not Bart Laton at all. Your name is Frank Sabine, and you lived in St. Louis, and you died November 16, 1843. Now, who are you?
A. Frank Sabine.
Q. Where did you live?
A. St. Louis.
Q. When did you die?
A. September 14, 1847.
Q. What was your business in St. Louis?
A. Banker.
Q. How many thousand dollars were you worth?
A. 750,000.
Q. Can you tell us something which Henry W. doesn't know?
A. Perhaps. I'm not a fraud.
Q. Who was mayor of St. Louis when you died?
A. John Williams,

At the next sitting, a week later, Henry W.'s father and mother, who were visiting him, were present, and a young lady named Miss J.

Q. Who is it that is writing?
A. Bart Laton.
Q. Where did you live?
A. Chicago.
Q. When were you born?
A. 1845.
Q. How old are you?
A. 50. [This sitting was held in 1895.]

In this and other answers where easy computations are correctly made, there is a slight hesitation accompanied by muscular indication of effort in the arm.

Q. Where are you now?
A. Here.
Q. But I don't see you.
A. Spirit.
Q. Well, where are you as a spirit?
A. In me, the writer.
Q. Multiply 23 by 22.

A. 3546.

Q. That was wrong; how do you explain your answer?

A. Guessed.

Q. Now, the other day you represented that you were someone else. Who was it?

A. Stephen Langdon.

Q. Where from?

A. St. Louis.

Q. When did you die?

A. 1846.

My question was in the form of a suggestion that he, the writer, is Stephen Langdon, which is naively accepted.

Q. What was your occupation?

A. Banker.

Q. But who was Frank Sabine?

A. I had the name wrong. His name was Frank Sabine.

Q. Now I want to know how you happened to take the name Laton.

A. My father's name.

Q. But where did the name Laton come from? Where did Henry W. ever hear it?

A. Not Henry W. but my father.

Q. (By Miss J.) Have you any message for any of us?

A. I don't know you well enough, but Prof. P—— should not be so incredulous about spiritualism.

According to Laton's later account of himself he was a tutor in a family in Chicago before the Civil War, where Henry W.'s father was a chore boy in the same family. Altogether inconsistent with this is his present statement that he doesn't know any of the company well enough to give them a message.

Q. But tell me how you came to assume the name Laton?

A. *I am a spirit.* (Written with great energy as heavily as the pencil would write.)

Q. What is your relation to Henry W.?

A. I am a spirit, and control Henry W.

Q. Of all the spirits why did you come to control Henry W.?

A. I was near when he began to develop.

Q. Now look here, this is nonsense. You are not a spirit, and you know you are not, and I must know how you came to pick up the name Laton.

A. Darn you, I am Laton.

Henry W. is allowed to read this, and, his father and mother being present, is greatly vexed and asks, "Did I write that?"

After this sitting Henry W. was absent for two years. Dur-

ing this time he never tried automatic writing, was never hypnotized, and apparently gave no thought to the previous experiments. The sittings were renewed in the spring of 1897.

Q. Who are you?
A. Bart Layton. [Note change of spelling from this on.]
Q. What have you to say to us?
A. Glad to see you.
Q. When did you write for us before? Give year, month, and day.
A. I don't know.
Q. In what year was it?
A. 1895.
Q. In what month?
A. Don't know. April, I remember. [It was June, 1895.]
Q. Tell us more about yourself.
A. I lived in Chicago.
Q. Do you live there still?
A. I am here now.
Q. How long did you live in Chicago?
A. Twenty years.
Q. Why did you leave there?
A. None of your business.
Q. In what year did you leave?
A. 1872.
Q. What was your occupation?
A. Doctor and carpenter.
Q. In what year were you born?
A. 1840.
Q. In what year did you die?
A. Did who die?
Q. In what year did you pass out?
A. 1875.
Q. Who was Stephen Langdon?
A. Chicago friend.
Q. Did you write Chicago friend?
A. Yes, can't you read?
Q How many minutes was it before you brought the book.
A. Five. (After hesitation.)

This question relates to the post-hypnotic experiment tried upon Henry W. two years before and related above. It was sprung upon the communicator to test his relationship with Henry W.'s hypnotic personality. The answers to the questions following about the fence are still more striking, for Henry W. never knew anything at all about the fence episode, having been tested after the experiment two years before.

Q. Where did you get the book?
A. Table.
Q. What did you do with it.
A. Gave it to you.
Q. Who else was with us?
A. Mr. Grimes. [Correct.]
Q. What was it you had to step over.
A. Fence.
Q. What kind?
A. Barb wire.
Q. Who was it stepped over the fence?
A. I did, you fool.
Q. What was your name?
A. Bart Layton.

The following questions and answers were from the last two sittings held two and three weeks later. At the beginning, an attempt, not very successful, was made to cultivate a good humor in the communicator. At the end, a second successful attempt was made to anger him.

Q. Who is writing?
A. Bart Layton.
Q. Good morning, Mr. Laton. Glad to see you. Would like to get better acquainted with you.
A. I don't care.
Q. Now, Mr. Laton, will you give us some message if you will be so kind?
A. From whom?
Q. Well, from yourself.
A. I am all right.
Q. From whom could you bring us a message?
A. Whom do you know?
Q. Well, I have many friends. Are you in communication with my friends?
A. George White.

In all Laton's writings this was the one single instance of the brilliantly intuitive type, though not a very striking one. I had an uncle named George White for whom I was named and who was killed in the Civil War. Henry W. knew nothing of this, but he had had opportunities of seeing my own name written in full, containing these two names with a third name, however, Thomas, between them. In answer to further questions, Laton said that George White was my father or grandfather and 'passed out naturally' fifteen years ago. Upon

a request for a message from George White, he wrote, He is glad to see you so well.

Q. Tell us, Mr. Laton, something we don't know, won't you?
A. Think you're smart, don't you?
Q. When did you write for us before?
A. Five weeks ago.
Q. Where have you been in the meantime?
A. Everywhere.
Q. Tell us something of your own life. How do you pass your time every day?
A. I never entirely leave Henry W., but partly so.
Q. When you leave him where do you go?
A. Anywhere or nowhere.
Q. What were you doing yesterday at this time?
A. With Henry W.
Q. What did you have for supper Thursday of this week?
A. None of your business.

Then followed questions in mental arithmetic in which my assistant and I both thought attentively of a certain incorrect answer. Wrong answers were given in each case, but not the ones we thought of. Laton was also asked to give the time of day, which in each case he gave incorrectly, even when we were looking intently at our watches.

Q. What was Mr. Laton's occupation in Chicago?
A. Carpenter.
Q. Two years ago you said he was a teacher.
A. Well, he—I used to be a teacher.
Q. Do you dance?
A. We don't dance who have passed out.
Q. Why don't you who have passed out dance?
A. You can't understand; we are only as you would say partly material.
Q. When you get through writing today, where is the part that is not material going?
A. It goes nowhere or anywhere as you choose to know space.
Q. Do you ride a bicycle?
A. Only through Henry W.
Q. Two years ago you spelled your name 'Laton.' How do you account for that?
A. Too many Latons; like the other better.
Q. I think you are an unmitigated fraud. What have you to say to that?
A. Shut up, you poor old idiot. Think I most always answer your danned old questions right? I can lie to you whenever I danned please.

This answer was accompanied by great muscular excitement of the hand and arm. There being one or two illegible words, I had the communicator repeat parts of the answer several times. The word 'danned,' evidently intended for 'damned,' was so spelled each time. Henry W., meanwhile, was calmly reading and never knew what had been written.

The automatic writing was now discontinued, as evidently there was little more to be gained from Laton. But the familiarity of the communicator with the hypnotic actions of Henry W. suggested one further experiment. If Henry W. were hypnotized, would the hypnotic personality assume the name Laton, and give the same account of himself orally? Henry W. consenting, hypnosis was induced by a few suggestions and was tested by a simple experiment in hallucination. I suggested that there was a five-dollar gold piece on the edge of the table. The subject saw it and asked whose it was. My assistant jokingly said that it must be Laton's, whereupon the subject went through the motions of grabbing it and putting it in his pocket with great glee, remarking, "If it's Laton's, it is mine, for he is a part of me." Evidently, then, the hypnotic personality did not necessarily consider itself as Laton, but my assistant's remark was perhaps a suggestion that Laton was not present. I therefore changed the subject's seat, bade him close his eyes for a moment and suggested that he was Laton. This was instantly successful, and a free conversation was then carried on with Laton as long as I wished. The subject's eyes were wide open and his manner easy and unconstrained, though not quite that of Henry W. There was no sign of Laton's recent anger, but the account that he gave of himself was the same as given in writing, with some added details. He said that he 'died' in 1875 at the age of sixty, that he lived on North Clark Street, that he was before the war a tutor in the family of Mr. Pullman, where Henry W.'s father was then a chore-boy, that he was a tutor of Mr. Pullman's little girl, but failing in the capacity of a teacher, and Chicago building up rapidly, he went to carpentering. He said further that he had been with Henry

W. since '75 ['95?], that he had chosen him because he was the right kind. "He developed," he said, "and I got a chance to show myself." A few other questions were asked testing the power of thought-transference, but without result. The subject was then awakened and found to have no knowledge of what had happened. A striking feature of the experiment was the instantaneous and naive assumption of the personality of Laton after the suggestion was made. As soon as the word was spoken, there was no confusion of 'he' and 'I' as relating respectively to Henry W. and to Laton.

Before commenting upon any peculiarities of the secondary personality indicated by the above conversations, I may mention some other details of the investigation. As may be seen, my attempts to trace from internal evidence the origin of the name, Bart Laton, were not successful. The external evidence yielded no better results. I could not learn that Henry W. or any member of his family had ever known any one bearing the name Bart Laton, or even Laton. The hypothesis that there was a real Bart Laton whose 'spirit' was communicating through Henry W. will hardly appeal to any one who has read the questions and answers, even if we grant, with Dr. Hodgson, that communicating 'spirits' must *a priori* be suffering from a certain amount of 'confusion,' or even 'aphasia' and 'agraphia.' The frequent contradictions as to the time of his birth and death, his uncertainty as to whether he was a teacher, carpenter or doctor, his willingness to resign his personality in favor of Frank Sabine or Stephen Langdon, together with the unmistakable evidence that the whole 'history' was progressively constructed in answer to my questions, make such a view as improbable as it is unnecessary. I did not, however, omit to make diligent inquiries in Chicago. The experiments were completed before Mr. Pullman's death, and through the kindness of Hon. Frank Lowden, his son-in-law, I learned that none of Mr. Pullman's family had known any one bearing the name, Bart Laton, that Mr. Pullman's daughter had never had a tutor by that name or any other male tutor. The chronology given by the communica-

tor would in any case make such a relation impossible. The communicator's statement that Henry W.'s father was at one time a chore-boy in Mr. Pullman's family was correct, but this was known by Henry W. and may indeed have served as a basis for the communicator's romance. I conclude, therefore, that the origin of the name is to be traced directly to the constructive imagination of the secondary personality.

In attempting any description of the marks of the secondary personality, either from a study of this or of other cases of automatism, we are struck perhaps first of all by the remarkable activity of the constructive imagination. Quite independent of all theories, the presence of this particular form of mental activity is characteristic. It is shown in this case throughout the whole conversation, for instance, in the fictitious answers to the mathematical problems, in the construction of the Chicago 'history,' and in the invention of the names, Mary Peters, Lucy Williams, Stephen Langdon, John Williams, etc. Frank Sabine differs from the others only in this, that I invented it myself and suggested it to the communicator. By way of experiment, any number of such names, some commonplace like John Williams, others more unique like Bart Laton, may be collected by any one who will ask a number of his friends to assume or invent a name on the spur of the moment. If, for the sake of the argument, we omit the comparatively few instances of the brilliantly intuitive type, the great mass of automatic utterances in this and in all other reported cases reveals the activity of the constructive imagination and shows further the most rigid adherence to the law of limitation to the store of memory images possessed by the subject. This limitation is painfully apparent in the utterances of my subject. The communicator has a vivid imagination, but the materials are all drawn from the experience of Henry W. The hypermnesia exhibited by many subjects and shown in a very trifling degree by mine—as, for instance, when Laton mentions one more of the Chicago streets than Henry W. can—in no way, of course, violates this law.

The suggestibility of the secondary personality is also apparent from this case. The communicator is willing, in response to my suggestion, to change his whole personality, and become Frank Sabine of St. Louis, and then proceeds to construct a 'history' consistent with the suggestion. In response to my suggestion again, he accepts the name Stephen Langdon, at another time becomes a good penman, admits that he 'guessed' the answers, etc. His suggestibility is limited only by a sort of insistent idea that he is a 'spirit,' which determines the answers in the form of a 'spirit' personality limited to the scant knowledge of what such a personality should be, possessed by Henry W. The very opposition which he shows in the later sittings is apparently the result of my indirect suggestion of hostility shown by the skeptical and disrespectful attitude which I assumed. In this connection, it is worthy of notice that in any conversation with a secondary personality, the questions themselves form a series of suggestions, and that properly prepared questions are of first importance. In the present instance, my questions may have determined the whole 'history' of Laton, and a different set of questions would have resulted perhaps in a totally different account. My first question, Who are you? really suggests a doubling of the personality. My question, Are you alive or dead? suggests perhaps the 'spirit' idea. The questions were well adapted to the study of the birth and development of a 'spirit' personality, but it would be interesting to know what a wholly different set of questions would have produced. We should observe that the question, Who are you? or, What is your name? is an indirect suggestion of a doubling of the personality. My first question might then have been, not, Who are you? but, Write your name in vertical script. If then the communicator had given the name, Bart Laton, I might merely have expressed surprise that his name was not Henry W., thus avoiding any even remote suggestion of a 'spirit' presence.

Another peculiarity of the secondary personality which has been noticed in other cases is its rather low or 'common' moral

and intellectual tone. This was conspicuous with Laton as well as with the other communicators mentioned in this paper. In the case of Laton, my skeptical attitude was assumed for the purpose of allowing this trait to develop and to see what kind of language the communicator would use when angered. Stupid profanity was the result. The answers throughout were commonplace. When asked for a message from the 'spirit' of my uncle, he can only say: "He is glad to see you so well." This peculiar trait is strikingly illustrated in one of my other subjects, the young girl mentioned above. To test her alleged clairvoyant powers, I had prepared a name written upon a sheet of paper and sealed in an opaque envelope. The communicator, the 'spirit' of the girl's deceased mother, professed to be able to read it and said that it was 'Mamie Nolds.' This was wholly incorrect, and I so stated. The communicator, however, insisted and insisted again that the name was 'Mamie Nolds.' I therefore opened the envelope, held up the writing, and triumphantly asked, "Now what have you to say?" To which this interesting and characteristic answer was written, "I think you are furrucht in the kopf," misspelled school-girl slang of rather a low order, such as I think the subject herself would not have used even with her associates. The utterances are sometimes of a flippant tone. One of the 'controls' of the girl just mentioned, professing to be the 'spirit' of 'Ben Adams,' who passed away in 1872, always wrote flippant answers. For instance, his veracity being questioned, he wrote, "I am not a fraud or a frog either." Asked the day and month of his death, he said, "I don't know. I got hit on the head."

Among the peculiarities of the secondary personality we may, perhaps, regard as fourth in order the brilliantly intuitive character of a very limited number of these utterances. In the case described by Dr. Hodgson these are very striking. With my subjects I have mentioned two instances of such utterances. Even with Bart Laton there is, as it were, a trace of the presence of such a trait in his mention of George White. Considering the sluggish character of Laton's mind and his

very slight ability to use the latent memories of Henry W., it does not seem very probable to me that Laton was shrewdly using a latent memory of a part of my name, hoping that it might happen to coincide with the name of some deceased relative. Such an explanation is possible, or it may have been a chance guess, but, considering the large number of such cases which the history of automatism affords, it seems to me better to note this power of happy intuition as one of the marks of the secondary personality. The explanation of it is not within the purpose of the present paper. It seems like the flickering survival of some ancient faculty. One thing only is sure in this case, the origin of the utterance was with the immediate participants in the experiment. For let us suppose that it was not a guess nor the revival of a latent memory of Henry W., but that it was communicated from some outside source. We should have to choose then between its being communicated unconsciously by me and its being communicated by the 'spirit' of the deceased George White. Put in this form, the 'spirit' hypothesis immediately becomes absurd, for, even if we have to assume, as is not indeed really necessary, that the name was communicated 'telepathically' by me, we must assume this and a great deal more if it was communicated by George White. Furthermore, if I may risk taxing the patience of the reader by further reasons where none are necessary, it would be more probable that the suggestion came from me from the fact that I have always had a romantic interest in the memory of this uncle, while George White, himself, hardly knew me at all. To my mind, however, rejecting the 'spirit' hypothesis does not mean accepting that of 'telepathy.' When the characteristics of the secondary personality become subject to accurate scientific description, some other hypothesis may be found quite apart from either.

Meanwhile it seems to me of the highest importance that the dignity of psychological science should be maintained by the use of modern logical methods. For instance, it seems to be regarded as a 'test' of the 'spirit' hypothesis by those who have urged it and to be naively accepted as such by reviewers

and critics, when the communicator is able to relate that which is occurring at a distant place. The instances of this seem always to have some element of uncertainty about them. But granting that such uncertainty were removed, what then? Eliminating fraud, and telepathy from those present, it is argued that it must then be 'spirits.' Imagine such methods pursued now in the physical sciences! Any new manifestations or reaction not following known laws might be attributed to 'spirits'! For instance, light-rays do not penetrate opaque substances. The new X-rays ignore this law; they must be due to 'spirits.' But, it may be said, we have, in the case reported by Dr. Hodgson, other tests of quite a different kind, where only the 'spirit' hypothesis is applicable. The one which appears to be particularly convincing is that of a communicator who gives as his name that of a New York man known to have died some time before, and who offers various convincing proofs of his identity. But is the logical aspect of this kind of evidence any better? Again, supposing that fraud, and telepathy from those present, are eliminated (and from the published reports, fraud at least seems to have been conscientiously eliminated), the bare facts of the case are that a certain woman in the city of Boston, in a certain abnormal condition, writes or relates occurrences which happened not only at a distant place, but at a past time, and shows herself familiar with certain friends and doings of the New York man. This is more 'remarkable' even than light-rays piercing opaque substances. Surely it must be due to 'spirits'! It may be that science will ultimately gain such knowledge of disembodied minds that it can use them as the basis of an explanation of phenomena in abnormal psychology, but at present the advancing of such hypotheses by psychologists can only serve to further the cause of superstition, to which people are already only too willing to fly when something mysterious presents itself.

As regards the various traits of the secondary personality, some of which have been referred to in this paper, it has been suggested by Mr. Podmore and others that they are instances

of survival or reversion. One cannot indeed fail to be impressed by the similarity of these traits to what we know or conjecture about the primitive mind. The general low moral and intellectual tone of the communications, the vulgarity and mild profanity, the frequent impersonation of the medicine-man, Quaker doctor, Indian doctor, etc., the keen memory and dull reason, the vivid constructive imagination, the deception and prevarication, the unwavering belief in spiritism, and the superstitious devotion to amulets, trinkets, and petty articles of ornament or apparel, all point to an early stage in the evolution of mind. Even the peculiar intuitive power sometimes exhibited by the secondary personality may be compared to the superior intuition of woman, whose mental peculiarities are in general representative of the more stable, basal and abiding phenomena of mind. Both may point to some nearly extinct faculty no longer serviceable. Still other peculiarities suggest the same theory, such as the extreme suggestibility and motor force of ideas, marks of automatism and of the hypnotic state, and at the same time characteristic of the child and savage mind. In close relation to this is the peculiar intimate connection between ideas and organic, nutritive and circulatory processes, best shown in hypnosis, and common to this group of phenomena. In view of such facts as these, certain of the more simple physiological theories of double personality gain considerable plausibility, such, for instance, as the revival of disused and outgrown brain tracts, particularly perhaps those of the less specialized hemisphere. The frequent appearance in automatic writing of *Spiegelschrift*, which occurs also among children, lends some support to this view.

It has not, however, been my purpose in this paper to propose any new theory or establish any old one to account for the phenomena of automatism, but rather to urge the extension of experimental inquiries in this direction, to point out certain prevailing peculiarities of the secondary personality, and to insist that the more complex and mysterious cases are to be understood by a constant reference to the simpler ones.

NEW PSYCHOLOGICAL APPARATUS.

BY

C. E. SEASHORE.

I. A SPARK CHRONOSCOPE WITH ACCESSORIES.

Of the hitherto known forms of apparatus for measuring short intervals of time, the graphic spark apparatus is the most accurate and the pendulum apparatus the most convenient. In the chronoscope that is shown in the accompanying figure, the spark method of recording is combined with the pendulum action.

The cut is reduced to a scale one-sixth of the size of the apparatus. The pendulum is shown in the starting position. The lower bob terminates in a knife edge which rests upon the projecting edge of a mechanical release key. The action of this key is soundless and gives the pendulum no impetus in either direction. On the other side of the apparatus is a spring key which catches the pendulum at the end of the swing. When the pendulum is released from this, it swings back with little assistance to the starting point and makes all necessary adjustments automatically. On the back of the lower bob is an index point which runs at the upper edge of the scale and serves as a spark point.

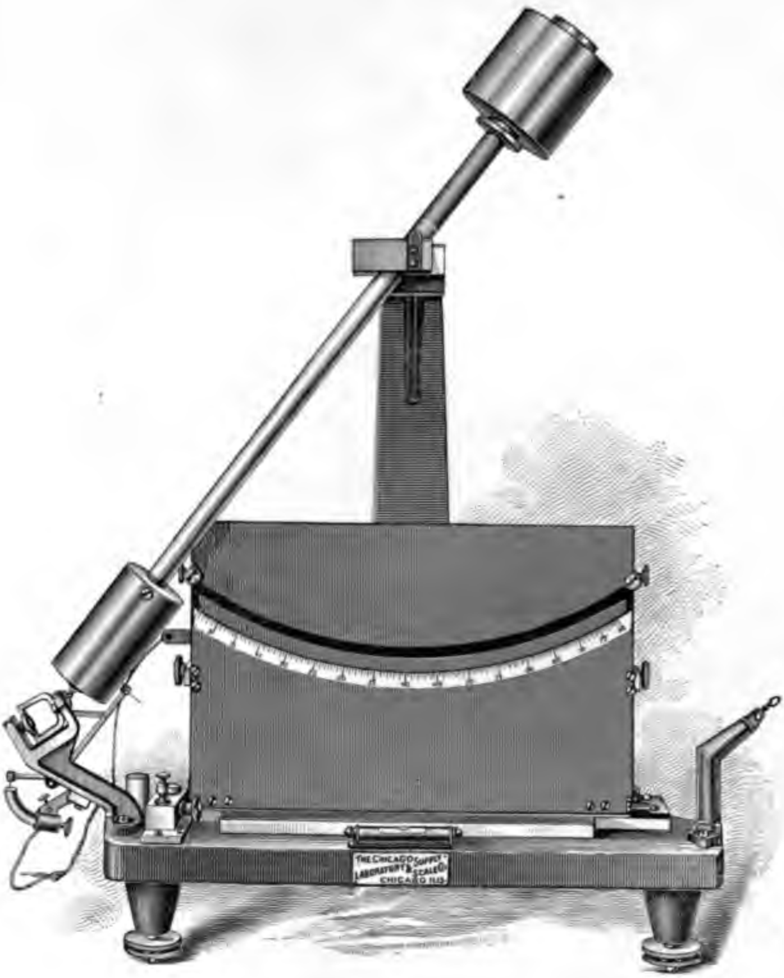
The record is made upon a smoked paper which is seen through the slit above the scale. This paper is stretched upon two rollers; it also rests upon an insulated metal plate which serves as an electrode and keeps the paper straight and smooth back of the scale. Back of this plate is a third roller by means of which the tension of the paper may be adjusted. The paper support is built on a carriage so that it may be removed and replaced without disturbing the rest of the apparatus. In preparing the paper this carriage is removed

and the paper is smoked as on an ordinary kymograph drum. As a complete record consists in a single spark which may be recorded at once, several hundred records may be made with one preparation of the paper, which may then be re-smoked so that a single paper may serve for several thousand records. The paper is moved as needed by a thumb screw at one end of the upper roller.

In reaction experiments the stimulus is given automatically by the apparatus when the pendulum indicator passes the zero point on the scale. A double rocking lever at this point makes one circuit and breaks another, either of which may be used in giving the stimulus. These contacts are adjustable platinum and mercury contacts and their adjustment may be verified by direct sight. The closing of the circuit is soundless, and the stopping of the lever in a soft rubber clutch makes no sound that can be heard a few feet away.

The reaction, or termination of the interval to be measured, is indicated by a spark on the sensitive paper at the edge of the scale. The spark is produced by interrupting the primary circuit of an ordinary induction coil. One secondary terminal is connected with the insulated plate on which the paper rests and the other is connected with the body of the apparatus. The point of the pendulum indicator is the nearest metal to the plate; therefore the spark flies from this point, through the sensitive paper, to the plate.

The scale is graduated empirically by the most reliable graphic method into hundredths of a second, and each unit is divided into halves. The average space of one unit is 5 mm. on the arc of the scale. With this adjustment the scale covers 0.80 sec. and the records are read in half-hundredths with ease and accuracy. This division is the most convenient and appropriate to use in reaction experiments. The variation in the movement of the pendulum is negligible because the pendulum is carefully constructed and balanced and moves without friction. The variation in the make contact is also negligible because the platinum terminal moves much faster than the pendulum indicator. The spark tends to take the short-



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est course between the point and the plate, but it may be deflected. The maximum distance between the spark point and the paper is 1 mm. The maximum deflection of the spark may be estimated to be about 45°. That amount of deflection is not liable to occur for the maximum distance, but if it did the maximum variation would be ± 1 mm. on the scale which is equal, on the average, to ± 0.002 sec. The average distance between the spark point and the paper is about ½ mm. and the average angle of deflection of the spark is less than half of 45°; therefore the average variation in the spark is less than ± 0.001 sec.

The chronoscope may be adapted for the measurement of longer intervals, as in the study of association, by two minor changes which can be made in a minute. A small weight is fastened on the top of the upper bob. This makes the pendulum swing so slowly that it takes three seconds to cover the arc of the scale. A corresponding scale, graduated empirically in hundredths of a second, is clamped over the regular scale. The accuracy is nearly proportional to the speed of the pendulum.

Similarly, if there should be a demand for finer readings than those obtained by the standard adjustment, an extra weight may be placed on the lower bob that will cause the pendulum to cover the arc of the scale, for example, in one-third of a second. If the corresponding scale is graduated in thousandths of a second each unit will occupy, on the average, 1 mm. of space. The degree of accuracy will be nearly proportional to the speed, because the latent time of the spark is negligible and the action is frictionless.

Much of the value of a chronoscope lies in its adaptation to the attachment of a variety of accessories. In this one the operator has the choice of using the automatic make or the automatic break. For auditory stimulus, I use a telephone receiver, with or without an induction coil, and in the make or the break circuit. For visual stimulus I use the apparatus described on p. 66 of this volume, in the make circuit.

For the study of associations, etc., with visual stimuli, I have

constructed a new tachistoscope which is adapted for use with the chronoscope. A face board contains an aperture 2 cm. high and 4 cm. long. Back of this is a double electric shutter which opens to expose the word or object to be seen. When the circuit in the electromagnets which keep the shutter closed is broken, one leaf of the shutter is drawn up and the other down by the action of two pairs of springs. In the opening, straight springs co-operate with coil springs making a quick exposure, but after the leaves have passed the aperture the former counteract the latter and thus minimize the vibration and sound of the shutter. The shutter exposes one section of a chance-wheel in which a series of cards may be inserted. The time for the exposure of any particular area can be readily measured with the chronoscope. This apparatus is also adapted for use in the usual tachistoscopic experiments in time exposure. It may be connected electrically with a pendulum which breaks and makes the circuit at measured intervals. This tachistoscope, like the chronoscope, has been made by the Chicago Laboratory Supply and Scale Company.

The following may be mentioned as special merits of this chronoscope: accuracy, adaptation for a variety of connections, soundless action, direct reading, ease and permanence of adjustment, and quickness and convenience of manipulation.

II. An Audiometer.

There exists a great demand for an instrument by which to produce and measure relative variations in the intensity of sound. Many instruments and methods have been devised for this purpose, but aurists and psychologists to-day measure the sound stimulus in terms of "my watch" or "my voice." The audiometer shown in the accompanying cut has been constructed primarily for use in the measurement of keenness

of hearing.[1] It is adapted to the needs of the psychological laboratory, the school room, and the aurist's office.

The essential and unique feature of this apparatus consists in the method of varying and measuring the relative intensity of the sound. This is accomplished by applying the principle that, for certain given relations between the primary and the secondary coils of an induction coil, the induced current varies directly with the number of turns of wire in the secondary coil. The complete apparatus consists of an induction coil, a battery, a galvanometer, a resistance coil, switches and a telephone receiver, all except the receiver being built into one compact and portable piece.

A dry battery is so connected that it may be thrown into the primary circuit of the induction coil by turning the left-hand switch. The galvanometer, seen through the crystal in the center, may be thrown into circuit by turning the right-hand switch. The fall of potential over the primary coil is reduced to the standard, e. m. f., by varying the resistance by means of the plugs at the farther end of the chest and gauging it by the galvanometer. The resistance permits of as small variations as can be detected by the galvanometer; and the galvanometer detects smaller variations in the current than can be detected by the ear at the receiver. The lever at the near end of the chest is a key which is used for the rapid closing and opening of the primary circuit in producing the stimulus. No current is drawn except for the moment that the circuit is closed by this key. The primary coil is longer than the secondary. The latter is wound in forty sections, arranged in a series according to the number of turns of wire that each contains, as may be seen in the accompanying table. Each of these sections is so connected with the surface terminals along the scale that the spring contact on the sliding carriage throws into circuit the number of sections indicated by the numbers on the scale. Therefore, to vary the energy communicated to the receiver in this circuit, it is necessary

[1] I am indebted to Mr. Charles Bowman, instructor in physics in this University, for much valuable aid in developing this audiometer.

only to move the carriage along the scale to the proper terminal. As it is most convenient to vary the stimulus in a geometric ratio according to the psycho-physic law, this principle has been taken as a guide in determining the scale of intensities of the sound. The numbers on the audiometer scale are given in the first column in the accompanying table; these indicate the corresponding number of sections involved in the secondary circuit. The second column gives the corresponding number of physical units in terms of the total number of turns of wire in circuit. The ratio of the increments in the sound is such that the forty steps in the series are, as nearly as can be determined, psychologically equal. The serial numbers on the scale are used in all readings. These measurements all refer to the strength of the current which energizes the receiver. The functional relation between the strength of current and the amplitude of vibration in the receiver is somewhat complex, but for the present purpose it may be regarded as fairly constant.

Table of Values for the Audiometer Scale.

I	*II*	*I*	*II*	*I*	*II*	*I*	*II*
1	1	11	13	21	58	31	270
2	2	12	15	22	68	32	315
3	3	13	17	23	79	33	368
4	4	14	20	24	92	34	429
5	5	15	23	25	107	35	500
6	6	16	27	26	125	36	583
7	7	17	32	27	146	37	680
8	8	18	37	28	170	38	793
9	9	19	43	29	198	39	925
10	11	20	50	30	231	40	1079

I, **scale on the audiometer.**
II, **corresponding values.**

The range of the intensity of the sound is such that it is not probable that any person can hear the weakest sound and all who can hear ordinary conversation at all can hear the strongest sound. The average threshold for normal ears lies near the middle of the scale. No delicate parts of the appara-

tus are exposed, and the battery, which is practically the only part that deteriorates, can be replaced without disturbing the adjustments. So long as the apparatus remains intact, the given relative measurements are constant for the same audiometer. Furthermore, the Chicago Laboratory Supply and Scale Company, which has taken great pains to perfect the mechanism of this apparatus, has one standard piece by which all audiometers made by them are standardized. Thus all who use this audiometer have a common standard and unit of measurement.

The relative scale of the intensities of the sound, the means of retaining the standard, the compact form, and simplicity and convenience of manipulation, are some of the merits in this apparatus.

For certain tests by aurists and experiments in the psychological laboratory, it is desirable to have a tone instead of a click for stimulus. Provision has been made for the production of tones in the audiometer. The inside connections are so arranged that by attaching a double contact electric tuning fork to the binding posts seen to the right, the fork may be made to interrupt the primary circuit of the audiometer and thus produce the tone of the fork in the receiver. This tone may be varied and measured in the same way as the regular stimulus.

University of Iowa

Studies in Psychology

EDITED BY

GEORGE T. W. PATRICK
PROFESSOR OF PHILOSOPHY

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CONTENTS

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Iowa City, May, 1902

A METHOD OF MEASURING MENTAL WORK: THE PSYCHERGOGRAPH [1]

BY

C. E. SEASHORE

In his presidential address at the meeting of the Psychological Association a year ago, Professor Jastrow outlined some problems in psychology that await solution at the present time. One of those problems is in part the subject of this paper, and I quote the following general statement of the needs, the possibilities, and the significance of this work:

"An adequate set of tests of normal functional efficiency, that shall receive a considerable authoritative sanction, is a great desideratum for the present-day needs, and an end by no means beyond the goal of properly directed endeavor. Its starting point is a correct analysis of the most distinctive modes of exercise of the several elementary components of our mental functions; the second step is the devising of tests that shall most simply, naturally, and definitely measure the functional efficiency of a selected factor or process; this accomplished, the way is prepared for the extensive utilization of such standards or norms of efficiency, (a) by their correlation with one another, (b) by a comparison with similar results obtained upon children at different stages of their development, thereby gaining an insight into the order and nature of their generic unfoldment, and (c) by a comparison with irregular, undeveloped, defective and decadent forms of such processes as they occur in connection with individual variation, with the

[1] **Read before the American Psychological Association Jan. 1, 1902.**

consequence of mental stimulation, or in disease. This program, which could readily be expanded, is even in outline a most extensive one—rich in detail, fertile in mutual suggestiveness of its parts, possibly momentous in its practical consequences. The conclusion is obvious that for a host of comparative purposes the determination of norms or standards of functional mental efficiency is indispensable. That such determination involves conventions and artificialities is true and proper and inevitable. But neither is a foot, nor a meter, nor a candle-power, nor a volt, nor an ohm a natural and predestined *ding-an-sich*. Yet the arbitrary and conventional character of these units does not interfere with their utility. I am not advocating a ready-made mental yard-stick which shall show in what measure all men are not equal, and how each may discover the thumb marks of his individual success or failure. All this has been attempted before, and with necessarily futile results. The problem is recognized to be one of a general statistical nature, freighted doubtless with practical consequences, but the application of which must always be uncertain and dependent for its success upon judgment and insight.''[1]

These functional tests may be divided into two classes. To the first class belong the tests on single acts, such as an act of discrimination, memory, or voluntary control. The commonest of these are tests on the senses. To the second class belong tests on the repetition of one or more such processes, with continuous effort and without interruption, for seconds, minutes, or hours. Such continuous exertion of effort is commonly called mental work, and the amount of work done is estimated in terms of the results accomplished. The tests which I shall describe belong to this second class.

What is mental work, and in what sense can we speak of

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[1] JASTROW, Psychol. Rev., 1901, VIII, 12.

measuring it? These two questions are naturally suggested by the title. Work is usually defined in terms of physics. By analogy we naturally think of mental work in terms of mental energy but an attempt to define this would lead us into endless difficulties. I therefore simply beg leave to use the expression mental work in the popular acceptation of the term, without involving any metaphysical implication concerning the expenditure of energy or any explanation of the causal relation between mental and physical processes. In the same manner, I must use the term measure in the freest sense. The idea of quantity is involved when we say a man solved ten problems, learned thirty verses of poetry, or settled five cases of dispute in an hour. But we should hardly speak of measuring these results, because the common units are too indefinite. The problems were not equally difficult and the man profited by practice; the verses differed and were not equally well learned; and the cases involved many variables. The determining of the amount of such work has ordinarily no scientific value because we cannot define the processes or control the conditions. We may speak of measuring the results of an act in proportion to the extent to which it is well defined and constant, and the conditions are known and controlled. The following description will show in what sense this can be accomplished. In professing to measure mental work, I simply attempt to answer with tolerable accuracy four questions in regard to the mental working capacity under given conditions: What? How much? How well? With what variations?

In dealing with this problem we are yet on the stage at which we must labor to perfect methods and means of measurement. The real problem will lie over, and many new ones will arise, while the experimenter devises tests and engages in a critique of method. Good progress has been made in the development of the first class of functional tests, but I am not aware of any generally accepted

test for the second class. Nor dare I hope that the principles herein set forth will prove a panacea. During the last three years I have devoted some time to the study of this principle of measuring mental work. One of the results is the elaboration of a principle of measurement and the devising and testing of apparatus for the purpose. The apparatus has received the descriptive name *psychergograph*, because it is a means of measuring mental work in the sense indicated above. In order to make the description clear and concise it will be necessary for me to make direct and unqualified statements and beg in return of the psychologist to supply self-evident qualifications.

In the designing of the psychergograph, I set myself the following aim: To devise means by which it shall be possible (1) to call forth a relatively simple and definite complex of mental activity, (2) to repeat the same for any desired length of time without interruption, and (3) to measure (a) the amount of work done, (b) the time taken, (c) the quality of the work, and (d) fluctuations in speed and quality.

A simple case of discriminative action seems best to serve as object of measurement. As a typical setting, I adopted the following: *Given one of four known signals, to recognize it and make the corresponding one of four simple responses.*

The apparatus consists of two distinct parts, the stimulator and the recorder. The stimulator makes a series of quick exposures of different signals. The observer is required to respond to each signal by a selective reaction. Every reaction brings out a new signal. The recorder makes a permanent graphic record. The two parts of the apparatus are seen in Fig. 1, the stimulator to the left and the recorder to the right.

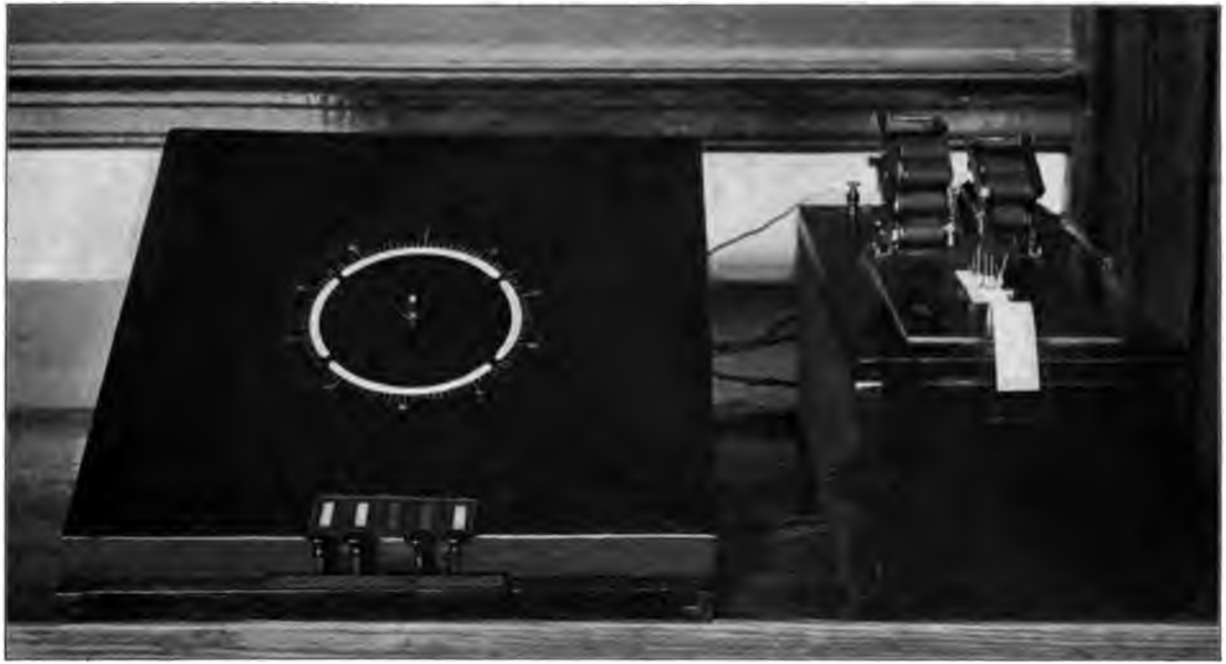

Figure 1. The Psychergograph

The stimulator is seen as a plain case, 40 cm. square, with a slanting cover. Near the front edge of the cover is a signal window, 8 mm. wide by 20 mm. long, through which the signals are seen. One hundred signals are pasted or printed on a paper disk, 38 cm. in diameter, so that when the disk revolves they are seen singly in succession right back of the signal window. The paper disk is clamped on a metal wheel which has fifty teeth on its edge. This wheel is energized by a strong clock spring which revolves it and the disk one hundredth of a revolution, thus exposing a new signal, every time the detent which holds it is released. This detent is in the form of a lever escapement and is operated by electro-magnets. The manner in which it operates will be understood after the reaction mechanism has been traced. Four reaction keys are seen back of the signal window. There is a signal index on the surface in front of each of these. When a new signal appears, the observer responds by pressing the key indicated by the signal, and the slightest movement of the key instantly causes the next signal to appear. The connection between the key and the signal disk is electrical. There is one pair of electro-magnets on each side of the escapement detent. These are connected, through a battery, one with each side of a small rotating commutator. The commutator is in turn placed in circuit with a battery and the series of keys. Whenever a key is touched, this circuit is broken and the commutator turns one notch, thereby switching the current from one side of the detent to the other. This alternating of the current causes the detent to oscillate and allow the signal disk to move forward by one signal space for every movement of a key.

A circle of the revolving disk is seen through the cover. On this there is a cross line (at 43 in Fig. 1), which passes before the circular scale of a hundred units and indicates to the experimenter which signal is in view. This indicator serves at once as a counter of the number of acts

performed and as a guide for the beginning and the ending of a series. The order of the signals is determined in the making of the series, either by chance or by some suitable system. The experimenter, therefore, knows the actual sequence of the signals in every series, but the observer has no means of knowing at any time what signal will appear.

The spring is wound by a detachable key that fits on the projecting axis. All the electrical connections are made through binding posts and switches on the back of the stimulator.

The recorder furnishes a continuous tracing of the action of each of the keys in the stimulator, and parallel to these, a time-line. The essential parts of the recorder are electro-magnetic markers, a record tape, a motor, and an interrupter.

The case of the recorder is 40 cm. long, 20 cm. wide, and 17 cm. high. Five markers are built up into a system on the surface, connected with batteries, and furnished with lead tracing points which mark five parallel lines upon the tape. An interrupter, to give a time-line, is placed in the circuit of one of these markers. The form and frequency of the interrupter depends upon how fine detail is wanted in the record. For seconds or half-seconds, some form of pendulum such as metronome or clock contact may be used; for tenths of a second the contact breaker made by the Cambridge Instrument Company may serve. Each of the other markers is connected electrically with a key in the stimulator so that whenever a key is pressed the circuit through that key is completed and the connected marker makes a jag in its line, thereby showing which key was pressed and the time relations of this act. Each marker is capable of three adjustments by means of thumb screws. The connections between the keys and their respective markers, and between the interrupter and its marker are made through the binding posts

on the top of the recorder and the back of the stimulator. The four markers have a common terminal and a single battery. There is a place for dry-cells inside of the case and either these or outside batteries may be used for the two circuits.

The record is made on common paper telegraph tape an inch wide. From a suspended spool in the case, it comes to the surface through a slit and is drawn under the writing points at a constant rate by a strong clock-work motor. The motor draws the tape by means of friction rollers and feeds it out free and in sight as it is shown in the Figure. In the present model the speed of the motor cannot be varied through a sufficiently large range. There should be three possible speeds, adapted respectively to the second, the tenth of a second, and the hundredth of a second reading. The motor is started and stopped by means of a button on the side of the recorder not seen in the Figure.

The apparatus is built in two pieces, in the first place, because it may be desirable at times to have them in separate rooms in order to reduce the disturbance. In the second place, this recorder—a multiple recorder—is useful for many other purposes in the laboratory. It is much more convenient than the kymograph in most cases of chronographic records in which the amplitude of the vibration is not an element in the measurement. In this respect it presents many points of advantage over the ordinary chronographs. Then, again, the stimulator may be used to advantage without the recorder in certain experiments. The total time of a series of acts may then be taken with a stop-watch and the trained experimenter can see directly whether any mistakes are made and what they are. The stimulator is then properly called a psychergometer or psychergoscope, as the amount of work done is read off directly by sight. This method can be employed only in certain crude forms of experiments.

Only a general scheme of the mechanism and the action

of the apparatus has been given. Its operation is simple. When a key is touched, a signal appears; the observer touches the corresponding key, and the marker connected with this key makes a jag in its tracing; the pressing of this key calls forth the next signal, and this process may be continued as long as one may desire. If the wrong key is pressed, the jag will occur in the wrong line. If there is any delay or interruption, this is shown by reference to the time-line. If there is any fumbling that will be shown by the duplication of jags.

From the experimenter's point of view, the operation of the apparatus is completely automatic. He has only to start the recorder by pressing a button and give the signal to begin. The personal equation of the experimenter is therefore completely eliminated. The records are permanent and may be read at leisure.

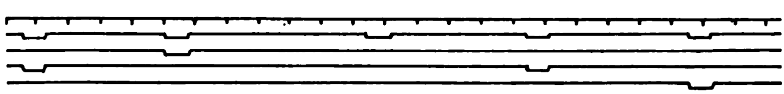

FIGURE 2. SECTION OF A RECORD

A section of a record is exhibited in Fig. 2. The upper line is the time-line divided into tenths of a second. The remaining four tracings are made with the markers so connected that whenever a key is pressed both the marker connected with that key and the marker next to the time-line respond. This double recording lends clearness and accuracy to the record. The time relations are all recorded on a single line, the one nearest to the time-line. The jags in the other lines simply show which key was pressed. The record represented in Fig. 2 shows that the keys were pressed in the following order: 3, 2, 1, 3, 4. Of course, when the first key is pressed there will be only one mark, as for the third act in the Figure.

A record of this kind is easily read. Take a long tape containing five hundred acts. It is first necessary to compare it with the standard series in order to trace the number and the nature of the errors. A list of the order of the signals is arranged as in the accompanying table, showing a standard record of a series of signals—or its equivalent, the required order of the responses. Here one hundred

A STANDARD RECORD.

1	2	1	3	4	1	4	3	1	2
1	3	2	1	3	4	2	4	1	2
1	4	2	4	2	3	1	2	4	3
4	3	2	1	2	1	2	1	3	4
3	4	1	3	4	1	4	3	2	3
4	2	3	4	2	1	2	4	1	4
2	3	4	1	4	1	2	4	1	3
2	1	2	1	3	2	4	3	4	3
2	1	4	3	4	3	1	2	3	1
4	3	2	3	4	1	2	3	2	3

acts are grouped by fives and by tens. The grouping into fives is made in order to facilitate the comparison. In the table one may see the order of five acts at a single glance and then, by a glance at the record, determine the errors. Thus, the fifth line of the first section of the standard table reads, 3, 4, 1, 3, 4; but the corresponding section of the record, shown in Fig. 2, above, reads, 3, 2, 1, 3, 4. This shows that an error was made in the second act; the response was made to the second signal instead of to the fourth. Should this error occur with exceptional frequency, the cause of it may be readily traced.

In order to determine the fluctuation of working power in a long series of acts, I have found it convenient to divide the acts into groups of ten by a check mark on the time-line after each group in the first reading. This is facilitated by having the table in the present form.

Three kinds of time measurements may be obtained by examination of the tracing of the first key, which lies adjacent to the time-line. First, the time of the entire series is found by counting the number of seconds from the beginning to the end. Second, it is economical, and important otherwise, to compare the time of regular groups of acts. A specimen table of successive groups of ten acts each is appended below. This is a very good record. The

TIME OF SUCCESSIVE GROUPS OF TEN ACTS EACH.

Observer, F. B. 497 acts. Total time, 296 sec.

6.0	6.0	6.5	6.0	6.5	6.0	6.0	6.0	6.5*e*	6.0
6.0	6.0	6.0	6.0	6.0	6.0	5.5	6.0	6.0	6.5
6.5	6.0	6.0	6.0	5.5	6.5	6.0	6.0	6.0	5.5
6.0	6.0	6.0*e*	6.0	5.0	6.0	6.0	6.0	6.0	5.5
6.0*e*	6.0*e*	6.0	5.5	5.5	6.0	6.5	6.0*e*	5.5	

observer shows remarkable self-control and power of endurance in continuous mental effort. The experiment was preceded by only two minutes of practice. The total time is only a little shorter than the average, but the constancy throughout the series is the distinctive characteristic. Only four errors were made. These are indicated by the small letter *e* for the groups in which they occurred. Most observers exhibit rhythmic fluctuations in speed and accuracy and characteristic variations with the progressive fatigue. Some work themselves into a "heat" and do better and better until some form of collapse occurs; others show progressive "disintegration" in loss of speed and accuracy, or in both. Third, the time of each individual act is obtainable. This may be taken in tenths or hundredths of a second and serves as a basis for the study of the variations within the group. In the specimen record

given, (Fig. 2), for example, the error in the second act caused a lengthening in the time of the third act.

What has thus been briefly described is the machine record. But every psychologist knows that this must be supplemented if any exact significance shall be ascribed to it. First, record should be made of the mental and physical condition of the observer before the test. The details of topics for these preliminary notes would depend upon the particular purpose in hand. Apparently trivial notes upon the previous and the present condition of the observer, if judiciously taken, may become complete explanations in the interpretation of peculiarities in the records.

Second, after each trial the observer must give an introspective account, as full as possible. He should especially try to account for the errors and describe the nature and the apparent causes of confusion states, periodic relaxation, progressive exhaustion, the struggle to inhibit automatisms, the intrusion of foreign trains of thought, and associations favorable or unfavorable. Experiments may be instituted for the express purpose of comparing the introspective account with the objective record.

Third, the experimenter has nothing to do but to watch the observer. He should make an objective study of conditions and effects, and should take private notes on the attitude of the observer in regard to interest, effort, strain, expressive attitudes, etc. He can make pencil checks on the record at the time to indicate parts to which the notes refer.

To pile up graphic records without some such supplementary notes as have been indicated would be sheer waste. The psychergograph gives us an accurate, unbiased record, but the real significance of this record depends upon our ability to account for the conditions which are elements in the process measured.

We may now turn to a brief outline of the possible vari-

ations in the experiments. The apparatus admits the introduction of a variety of signals. What the signal shall be depends upon the purpose of the measurement. It is often desirable to make the case as simple as possible. Then we reduce the difficulty of the discrimination to a minimum by making the signals as different as possible. We may take a color, a letter, a circle, and a dot. Or, we may take four readily distinguishable letters, geometric figures, pictures, words, or colors. In the test described thus far as typical, four clearly distinguishable colors were used. The degree of difficulty in the discrimination is a controllable variable and may itself be the object of measurement. Various degrees of small differences in the shade of colors, confusing geometric figures, or any other objects in which the degree of resemblance can be varied and described may be employed. Then again the higher processes in the act may be varied and complicated in many ways by requiring certain restricted associations, memories, judgments, and decisions based upon the recognized signal. In fact, all the variations and complications of the usual visual reaction experiment, both simple and complex, may be introduced, and provision is made for the uninterrupted and long continued repetition of the selected act.

The number of kinds of signals is not fixed. The apparatus makes it possible to use a hundred different signals, if so many should be wanted, as might be the case for example in a classification test in which the observer would be required to divide the whole series of objects into a small number of classes on the basis of certain class characteristics. But for most purposes it is best to use four signals, have an equal number of each kind, and have them of equal discrimination value.

Experiment has demonstrated that there is great advantage in using just four keys. The first model of this apparatus was made with ten keys. Unless one wishes to make the crudest kind of experiment, it is necessary to commit

to memory the order of the signal indexes before beginning the experiment. Ten proved entirely too confusing, because there was so great difference in the time that it took to find different keys, even after they had been well learned. Experiments were then made with six keys but even this was too many. Finally the system of four was adopted. In a series of discriminative actions like this, it is really necessary to have as many as four different ways of reacting in order to prevent anticipation, but very little is gained for this purpose by having more than four.[1]

The length of the experiment may be varied. The observer must work with a maximum degree of effort and there is no rest and no relief, because the work is uniform and continuous, requiring uninterrupted effort from beginning to end. Under these conditions a hundred acts is a great task and for many purposes that length of a series is satisfactory. But no one will learn the order of a hundred indifferent signals of this kind the first time he goes over them, especially if his effort be concentrated upon speed and accuracy. Furthermore, the observer has no means of knowing in what part of the series he begins or how he is progressing, because a small screen is placed in front of the scale. Therefore he may continue and go over the same series several times without learning to foresee a single sequence. Much depends upon what plan is fol-

[1] In the discussion of this paper before the American Psychological Association, Professor Jastrow stated that it was not necessary to construct a special psychergograph because a typewriter may be used, and he recommended the Oliver machine on the ground that it prints the letters in plain view. So far as I am able to see, there are close restrictions to such use. Suppose, as was suggested, that the labels on the keys are interchanged by pushing lettered paper caps over the keys. If only a small number of keys should be used, the observer would soon learn this little series within which no variation obtains. If, on the other hand, a large number of keys should be used, it would take longer to learn the series but gross confusion would ensue in hunting for the keys. The difficulty is this: the typewriter key always prints the same letter throughout the series, whereas the psychergograph key may call forth any one of the total number of signals. This is an essential and radical difference. There are, however, many tests on routine processes that may be made with the typewriting machine.

lowed in the arranging of the series. If the order is determined by chance, two or more signals of the same kind might come in succession and these might serve as a clew to memory. Some order like that shown in the specimen series given on page 10 is better. If several tests are to be made in each sitting and to be repeated on several days, a different series of the same kind may be used for each test. But very many repetitions of the same series may be made without any disturbance from memory, if proper precautions are taken. In fact for some purposes, it is necessary to use the same series in a large number of successive trials.

The index signals are placed at the same level as the signal window and the keys, and they are all so close together that it is not necessary to move the point of regard from the signal window in order to see the index and the keys. The position of the four indexes are, however, learned before beginning the test and, for long experiments with the same order of signal indexes, it is best to have them covered and trust entirely to memory.

One important means of varying the experiment, as in the study of habit, consists in changing the order of the index signals.[1]

The bodily movement in the process, the pressing of the keys, is reduced to a minimum and a constant.

This outline of the construction of the apparatus and the method of measuring mental work is reported apart from its connection with any specific problem of research partly because, like the chronoscope or the kymograph, the psychergograph is adapted to a variety of uses. One investigation now in progress with this apparatus and method is on the subject of fatigue.

There are certain objections to this apparatus; for instance, its action is not soundless, it requires batteries, and it is rather expensive. But if the principle of the experi-

[1] Some of these variations are suggested by Jastrow in his description of a sorting apparatus, Psychol. Rev. 1898, V, 297.

ment is correct, these objections may be overcome in time. This is the third model. The three have been built upon entirely different mechanical principles. In the first model, a column of celluloid disks was fed up to a signal window by a spring and at each reaction an electric hammer knocked away one disk, thereby exposing another. The same kind of record was obtained as in the present model, but through a simple mechanical action. This first model is so simple that it can be built in two days, by one man. I have used it successfully in more than a hundred experiments. For the one experiment, with colors, it is satisfactory, but it is limited to that use. The second model was a belt machine, the signals being carried on a belt which moved before the signal window. This proved unsatisfactory, probably on account of the imperfect workmanship in it. Thus there are many ways in which the main end may be accomplished. Then again, it requires only a minor step to make adaptations for the use of other sense stimuli, such as auditory or pressure stimuli.

The record may be called a psychergogram. It is truly a measure of mental work. First, it gives the amount of a particular work done. Second, it gives the time of each act, the time of small groups of acts as by fives or tens, and the total time of the series of acts. Third, it gives a quantitative expression to the quality of the work in terms of the number of errors, the sequence of errors, and the classification of errors. These are the mechanical elements in the record on the tape. Every feature of the record is a fact and these facts may be reduced to statistics, provided the conditions are observed and taken into account in the interpretation. He who wants the facts must labor to observe the conditions. There is a class of would-be observers who, when they see the beautiful automatic operation of the psychergograph, rejoice that they have at last found an automatic accumulator of statistics on mental processes. The psychergograph is not dedicated to them.

A VOICE TONOSCOPE[1]

BY

C. E. SEASHORE

The study of motor processes has remained comparatively neglected in experimental psychology. It is not that the motor processes, as compared with the sensory, are less significant, of a baser sort, so much simpler, or fewer in number. It is because the analysis of the one naturally precedes the analysis of the other. The study has been pursued in the natural order in this inceptive stage of the science, but interest now begins to center upon the study of the motor process, not only because the motor process is the outcome of, and the sequel to the sensory process, but also because it is the practical phase of life.

In the psychology of music, the time has now come to begin to turn from the study of the hearing of tones to the study of the singing of tones. But, as we have had need of delicate tone-giving instruments in producing and gauging stimuli for the sensory processes, we now need, in addition, the corresponding means of measuring the results of motor processes. The lack of such instruments is conspicuous. The unaided ear will not serve the purpose. The graphic and photographic apparatus in use are too cumbersome. It is difficult to obtain a simple and at the same time accurate and ready means of measuring the pitch of tones produced by the human voice.

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[1] Read before the Section of Anthropology of the American Association for Advancement of Science, Jan. 1, 1902. The name "tonometer" was then used instead of tonoscope, but it is apparent that the latter term is preferable because it distinguishes this instrument more completely from other tone-measuring instruments.

The apparatus that I am to describe is somewhat complicated in construction, but when once built it is easy of manipulation, can be used for rapid work, and can be made sufficiently accurate for all purposes of routine measurements for which it is adapted. It is intended to be used in measuring the pitch of the human voice in singing and speaking; and, since it causes the vibration-frequency, which denotes the pitch of the tone, to be *seen* directly when the tone is sung, this apparatus may pass by the descriptive name of voice tonoscope.

The voice tonoscope is constructed on the principle of the stroboscope; that is, the vibrations of the voice are made visible upon a moving surface by the action of intermittent light. I take pleasure in acknowledging my indebtedness to Professor Scripture, who first employed this method of measurement in an exercise on the reproduction of tones.[1] The apparatus is "built up"; the individual parts, such as the manometric capsule, the vacuum tube, the stroboscopic disk, and the double contact fork, are used in physics, but Scripture was the first to use them in this way for psychological purposes. The special feature of the present apparatus is the stroboscopic screen, shown in Fig. 3, but a brief description of the complete apparatus and of the methods of measuring by means of it will be given.

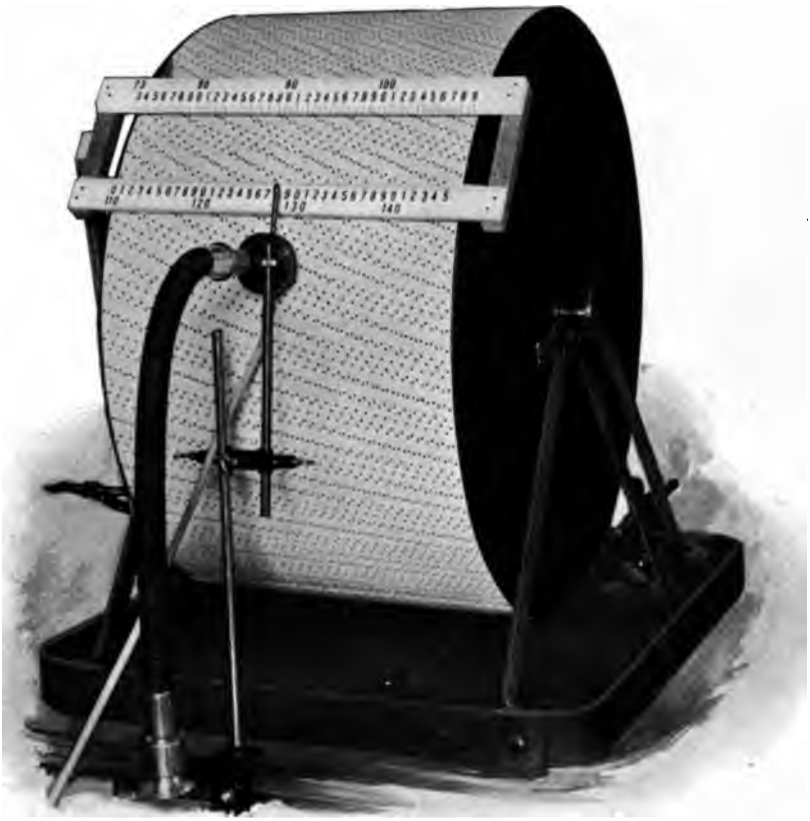
The following are the essential parts of the apparatus: (1) means of producing a standard tone, (2) a special moving stroboscopic screen, (3) means of projecting the vibrations of the standard tone upon the screen, and (4) means of projecting in a similar manner the vibrations of the tone sung. These parts will be described in the order in which they have been mentioned.

The standard tone, from which the singer takes his key

[1] SCRIPTURE, *Elementary Course in Psychological Measurements*, Studies from the Yale Psychol. Lab., 1896, IV, 135.

and by which the standard of measurement is determined, is produced by an electric fork in a distant room and is heard through a telephone receiver which is in shunt circuit with the fork. A switch makes it possible for the experimenter to sound the standard tone at required intervals of time; or the observer may himself regulate this roughly by moving the receiver to and from the ear.

The stroboscopic screen is made in the following manner: A metal drum 50 cm. wide and 50 cm. in radius is mounted on ball bearings and revolved by an electric motor. A heavy white paper is stretched around the drum and on this paper seventy-one parallel lines of dots are drawn. The dots are 3 mm. in diameter and equidistant and the lines extend through the complete circumference of the drum. The lines of dots are divided into two series. The first series contains thirty-six lines drawn equidistant and so spaced as to cover the whole screen. Their arrangement and the number of dots in each is shown by the numbers in the upper section of the scale seen in the Figure. The first line has 73 dots and each succeeding line in the series has one more dot than the line immediately preceding. The second series has thirty-five lines and these alternate with those of the first series. This arrangement and the number of dots in each line may be seen by the numbers on the lower section of the scale. As regards the number of dots in each line, the second series forms a direct continuation of the first and the dot-frequencies of the two series together, therefore, extend from seventy-three to one hundred and forty-five by a uniform increment of one dot for each line. The aim in the arrangement of the dots was to secure dot-frequencies which should correspond to the vibration-frequencies of all tones within the range of the human voice. In musical terms, we have here what corresponds to the vibrations of all tones within the octave 73 v. d. to 146 v. d. The stroboscopic effect is such that the dot-frequency for one



THE TONOSCOPE

octave will serve quite as well for tones in higher or lower octaves, as will be explained later.

The standard tone is reproduced to vision upon the screen. The accessories for that purpose are not shown in the illustration. On the back side of the drum, there is a scale like the one seen on the front side and occupying a corresponding position. A vacuum tube, 30 cm. long, is fastened at the upper edge of the scale and supplied with a reflector which throws the light on the scale and prevents it from shining over the drum. The terminals of this tube are connected with the secondary terminals of an induction coil. The primary circuit of the coil is completed through a battery and the double contact fork that produces the standard tone. The room is darkened in order to secure the intermittent light effect. Each vibration of the fork breaks the circuit and the resulting spark produces a flash of light in the vacuum tube so that the screen is lighted up once for each vibration of the standard fork. Suppose that the fork is a 100 v. d. fork and the drum is revolving at the rate of one revolution per second. Then, when the dots on the revolving screen pass under the intermittent light, the line which has one hundred dots appears to stand still and is clear and well defined, because the frequency of the dots is the same as the frequency of the lights: every time an image of this line is cast upon the retina the dots appear in the same position. The dots in all other lines appear blurred and moving, assuming more or less the appearance of gray streaks. It is therefore easy to detect the line that is wanted. The purpose of this contrivance for projecting the standard tone is merely to secure the means of regulating the speed of the drum. Thus, in the present case, the speed of the drum is regulated so that the one hundred-dot line stands still continuously. This line then becomes the standard for the visual scale in terms of which the measurements are to be made. Any other tone projected on the same screen in a similar

way will make some other line stand still and the pitch of that tone will be indicated by the number of that line, or a multiple of it; e. g., if the second tone makes the 105-line stand still, it is a tone of 105 v. d., or some multiple of that number, as 210 v. d. or 420 v. d. Which octave it is in can readily be told by the practiced ear but it is also shown by the formation of the dots.

The tone that is sung is projected on the same screen, in a similar manner, although by different means. In this case the intermittence of the light is produced by means of a manometric flame on the side of the drum opposite the vacuum tube. The manometric capsule, with the connected gas tube and the speaking tube, is seen in the Figure. The observer holds the speaking tube before his mouth in the way such tubes are usually held, and sings the tone that is to be determined. The tonal vibrations cause sympathetic vibrations of the little gas flame in the capsule so that the flame rises and recedes once for each vibration of the vocal organs. This gives an intermittent light like the one produced in the vacuum tube on the other side of the drum. When the screen is moving at the standard speed, that line of dots will stand still which has dots to correspond to the frequency of vibration of the tone, and the pitch of the tone is indicated by the number of the line that stands still. The pitch may be seen as soon as it is heard because the visual and the tonal effects are synchronous.

This summary description of the essentials of the apparatus also indicates, in a general way, the mode of procedure in experimenting. It reduces itself to this: the singer takes the key tone from the receiver and sings a tone before the speaking tube; the vibrations are projected on the moving screen and the pitch of the tone is seen on the scale.

Although the apparatus consists of several parts, only the drum with its screen is made for this specific purpose.

The other parts are such as are found in any ordinary physical or psychological laboratory, and need not be described in detail, but some further particulars about the construction and use of the screen are necessary.

The arranging of the lines into two series is important. It is necessary to have all the lines within a certain compass because the necessarily small light will not serve for more than a limited space and the spreading of the record over a large surface would therefore interfere with quick reading. This drum was made as wide as is practicable under the named restrictions. An attempt was made to put the lines in a single series and it was found that then only about thirty lines could be put on the screen and be read satisfactorily. This number of lines would cover only a part of an octave within the desired range of tones. At least seventy lines are needed. By the double series plan it is possible to put seventy-one lines on the space occupied by thirty in the single series plan; and it is easier to read on this screen of seventy-one lines than it was on the original screen of thirty lines covering the same space. The reason for this lies in the fact that by the double series plan no two adjacent lines have nearly the same dot-frequency; consequently the line on each side of the one that stands still appears as a gray streak and forms a sort of frame for the line one is reading. This has two effects: in the first place, the two series may be placed on the same screen without causing any interference; in the second place, the one series of lines makes the other clearer, so that it is possible to put the lines closer together. Having separate scales for the two series is also conducive to clearness. The same device, therefore, makes it possible to save space and add clearness and thus to provide for the desired range of tones.

Let us suppose that the speed of the drum is regulated by the 100 v. d. fork to make one revolution per second. Then, if the tone that is sung lies within the range of the

octave from 36 to 72 v. d., the line that stands still has twice as many dots as there are vibrations in the tone and, in the reading, the number on the scale is simply divided by two; e. g., if the 75-line stands still the tone is 37½ v. d. The ear readily reveals whether or not the tone lies within the low octave and these low tones can be sung only by the male voice. For tones from 73 to 146 v. d., the reading is direct from the scale. This represents the middle range for the baritone. For tones within the octave from 146 to 292 v. d., the line that stands still appears to have twice as many dots as it actually has. That makes it easy to recognize this octave and, in the reading, the number on the scale is doubled: e. g., if the 75-line stands still, the tone is 150 v. d. This octave represents the upper range for the male voice and the middle and part of the lower range for the female voice. From 292 to 584 v. d., there will be four vibrations for each dot in the line that stands still and the numbers on the scale must be multiplied by four. These multiples may be placed in small figures on the scale. The same principle of reading may be extended to higher octaves in so far as it depends upon the screen, but there is a limit set by the degree of sensitiveness of the diaphragm in the manometric capsule.

If a high tone is studied, it is well to run the drum at the rate of two revolutions per second. This can of course be regulated by the 100 v. d. fork. Higher forks may also be used for the sounding of the key-note after they have been tuned with reference to the 100 v. d. fork. The tonoscope may be used in tuning them.

It is only relatively true that a line stands still; it stands still only when there is an absolute agreement between the vibration-frequency and the dot-frequency. If the vibration-frequency is a fraction greater, the dots move gradually downward, and if it is less, they move gradually upward—and the speed of the apparent motion is always in proportion to the difference. Thus, if the tone is 125.5 v. d., no

line stands still, for the 125-line moves downward at the rate of one space per second and the 126-line moves upward at the same rate; but, if the tone is 125.1 v. d. the 125-line moves downward at the rate of only one-tenth of a space per second and the 126-line moves upward at the rate of nine-tenths of a space per second. The practiced observer can therefore readily learn to read in tenths of the scale units.

The accuracy of the reading varies with the accuracy of the singing. The accuracy increases with the steadiness of the voice, and hence with the demand for accurate measurement. The more firmly the pitch is sustained, the easier is the reading. The ordinary singer wavers in the tone that he tries to sustain even for half a second and the experimenter must select the predominating pitch in it.

The drum is heavy and carefully mounted so that it runs at a fairly uniform speed, but if the speed should tend to vary, the experimenter records such variations and a corresponding correction in the readings is made from a table which is exact to one-tenth of a vibration.

The tonoscope is not constructed for any one particular psychological experiment. It is intended to be a general measuring instrument that may be employed in a number of ways. It is simply a quick and accurate means of determining the pitch of tones—not only of the human voice, but also of instruments from which the sound may be projected upon it. In coöperation with a student, Mr. Edward Bechly, I have begun a study of certain facts about the nature of the control of the voice in singing and speaking. The results are not ready for publication but some of the measurements that we have made may be mentioned in order to indicate some purposes for which the tonoscope has been used with excellent success.

1. Sounding a single tone. This measurement presents different aspects according as the tone is taken at random, is heard from a standard, or is heard from a standard and

hummed before being sung. The absolute pitch, the intensity, the mode of vocalization, and the time relations are factors that may be varied in order to determine their effect upon the pitch of the tone.

2. Sustaining a tone. "Sustained" tones vary in pitch according to a general law. The factors that influence this may be determined.

3. Singing tones at certain pitch intervals. The standard tone may be given or it may be taken at random by the voice.

4. Singing musical scales, natural and chromatic, with numerous variations in the conditions.

5. Singing a melody. There is a shocking surprise in store for every singer who tests his ability to sing even the simplest air in true pitch. He finds that his good ear has tolerated great license in the matter of fidelity to pitch. Two-part harmony, or unison singing, may also be measured by recording for one voice on each side of the drum, but this we have not tried.

6. Speaking in a uniform pitch (a pure fiction).

7. The least producible difference in pitch. This is the measurement that is of greatest value for psychology. The least producible difference is to the study of motor processes what the least perceptible difference is to study of sensory processes.

AN ILLUSION OF LENGTH[1]

BY

C. E. SEASHORE AND MABEL CLARE WILLIAMS

Two years ago, while studying the æsthetics of geometrical forms, we required the students to produce 'double squares' by direct eye estimation. The forms were produced by means of an adjustable frame and were to be made twice as long in the horizontal direction as in the vertical. Not only was the illusion of the vertical counteracted, but there appeared to be a tendency to make the horizontal distance too short.[2]

We repeated the same test with two hundred school-children,[3] ten boys and ten girls of each age from six to fifteen inclusive, and found that this illusion of length is much stronger for children than for adults, and that it decreases with the increasing age of the children. Thus, the children of six made the double square 28% too short, the 7's 24%, the 8's 25%, the 9's 14%, the 10's 10%, the 11's 11%, the 12's 11%, the 13's 10%, the 14's 8%, and the 15's 9% too short. These figures seem fabulous, especially when it is remembered that they represent the amount by which the well-known illusion of the vertical has been outweighed. The illusion of the vertical was measured in the same series and found not to vary noticeably with the age of the children.

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1 Reprinted from THE PSYCHOLOGICAL REVIEW, VII., 592–599.

2 "Forty-eight made the horizontal line too short by an average of 15 mm. (mean variation, 8 mm.), and fourteen made it too long by an average of 10 mm. (mean variation, 6 mm.). On the whole the horizontal distance was made 4½% too short." *Univ. of Iowa Stud. in Psychol.*, 1899, II., 18.

3 *Op. cit.*, p. 33.

Continuing the study of this illusion in persons who knew little or nothing about the nature of the illusion, we made a series of measurements upon university students in introductory psychology, before the subject of illusions had been discussed in class. Twenty-nine men and thirty-four women volunteered to be tested. The so-called unconscious method was followed in so far as it was possible. Most of the observers, however, surmised that some illusion was involved in the test, and the conscious or subconscious reaction to such suspicion often entered into their judgments. The records of two men were discarded because these men asserted that they had attempted to eliminate the illusion of the vertical. The observers may evidently be divided into two classes, namely, those who introduce some correction for supposed illusions and those who do not. But as such division is relative and cannot be made satisfactorily either upon introspective testimony or upon inspection of the results, we group the records together and state some of the conclusions which may be drawn from the averages of the results.

The forms to be studied were cut out of cardboard and then placed on a neutral background upon a large drawing board, which was so placed that they were 50 cm. from the eyes and at right angles to the line of regard. Some of the forms were drawn on the background, as will be explained. Every effort was made to eliminate disturbing influences from color, brightness, limiting lines and spaces, movements in the adjustment of the cards and other known sources of error. The forms of figures may be grouped into five classes, as is shown in the accompanying table. They are: (A) parallelograms, (B) a vertical and a horizontal line, (C) and (E) two horizontal lines of different length, and (D) unequal horizontal distances limited by points.

In the A-series, rectangular forms were to be produced as follows: A1, a double square, double in the horizontal

direction; A2, a double square, double in the vertical direction; A3, a half square, the standard vertical; and A4, a square, the standard vertical. The standard for all was the side of a square, 114 mm. A card 114 mm. wide and about 275 mm. long was used, and the observer was required to produce the respective forms by placing a similar card over such portion of it that the remaining part would constitute the required form. The figures in the B-series are parallel to those of the A-series and consist of only two of the adjacent lines from each form of the parallelograms. They were to be produced as follows: B1, the horizontal line twice as long as the vertical; B2, the vertical line twice as long as the horizontal; B3, the horizontal line half as long as the vertical; and B4, the horizontal line equal to the vertical standard. The variable line was 275 mm. long, and the observer covered such portion of it with a card that the remaining part of it stood in

Form.																						
Case St.	A1 228		A2 228		A3 57		A4 114		B1 228		B2 228		B3 57		B4 114		C 228		D 228		E 228	
	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d	X	d
M	222	9	213	10	62	3	115	3	226	7	211	6	64	3	119	2	225	8	227	11	217	7
W	222	5	214	5	63	3	116	3	223	7	216	5	63	2	120	4	226	7	227	9	216	9
Ave.	222	7	214	7	63	3	115	3	222	7	214	5	63	2	120	3	226	7	227	10	217	8
%E.	–3	3	–6	3	+11	5	+1	3	–2	3	–6	3	+11	4	+5	3	–1	3	0	4	–5	4

Form, the general shape and the position of the forms.

Case, the designation of each case in this report.

St., the required length of the measured line or distance. The unit of measurement is the millimeter.

X, the average of the results of the estimates: (M) by men, (W) by women, and (Ave.) by all together.

d, the averages of the mean variations.

%E, the differences between the estimate and the standard, stated in percentages of the required lengths. The mean variation is also reduced to percentages.

The number of trials is two for each person upon each point in Cases A, B and D, and ten in Cases C and E. The double fatigue order was observed.

the proper ratio to the standard line. In Case C the two lines lie in the same direction (horizontal) and the measurement was made as in the B-series. The variable line was placed 10 mm. lower than the standard in order to eliminate the motive to connect them. In Case D the standard distance (horizontal) was given by two points. The observer was required to place a third point so far to the right of these that the distance between the second and the third points appeared to be twice as great as the distance between the first and the second. In Case E, the figure is the same as in Case C, but the observer was required to select the limiting point before applying the limiting card.

The final averages of the results may be seen in the accompanying table. Case A1 represents the original form of the illusion. The double square is made 3% too short. This indicates that the illusion of length exceeds the illusion of the vertical by that amount, because the two illusions stand in direct opposition in this position of the figure. If only the illusion of the vertical had been present, the double square would have been made too long. Case A2 shows the effect of the coöperation of the two illusions. Here the double square is made 6% too short. We have found, in a large number of other experiments, that the two illusions in coöperation produce an illusion equal to the sum of the two illusions acting singly; still that is not always to be expected, because the effect of one illusion partly satisfies the motive of the other. Case A3 was introduced as a check upon the method of making the comparison and the measurement. In shape this figure is identical with A2, but it differs from it in size, and was produced by varying the shorter dimension. This half-square was made 11% too wide. In all our experiments it is found that when the vertical distance is varied there is a stronger tendency to correct the known illusion of the vertical than when the horizontal distance is varied. This is because more atten-

tion is called to the presence of that illusion by the former method. That law does not apply to the illusion of length, because the observers had no means of knowing that such an illusion existed. As will be shown later, the difference in size does not account for the difference in the illusion for the half-square and the double square. Case A4 contains only the illusion of the vertical. The horizontal distance is made 1% too long, but that is not a representative figure. Some four hundred measurements upon students gave an average of 5% for the illusion of the vertical. It is difficult to say how far this tendency to correct the illusion of the vertical enters into the estimation of the double square in the present experiments. It is probable that it increases the difference between the two illusions in A1, and that it decreases the resultant of the two in A2 and A3.

We conclude from the study of the A-series, then: (1) that the illusion of length obtains for both the horizontal and the vertical positions of the double square, (2) that it is stronger than the illusion of the vertical, and (3) that the tendency to make unconscious corrections for an illusion is greater when the image of the illusion is clearest in the mind.

The B-series was so arranged as to determine whether the illusion is a linear illusion or is peculiar to the comparison of the two dimensions of rectangular areas. The relations of the dimensions of a parallelogram may be estimated, (1) by comparing a vertical margin with a horizontal margin, (2) by comparing the length of imaginary lines that cross at the center of the figure, and (3) by judging by the total impression with reference to the areal content without distinctly selecting representative lines. The first method is the easiest and the most accurate, and was followed by nearly all our observers. When the second method is employed, the illusion is increased on account of the increased vagueness of the parts to be compared. The

third method is the most valuable and probably entails the greatest illusion. Children in their quick and spontaneous judgments follow this method, as a rule, and this accounts partly for the strength of the illusion with them. When the first method is employed we have essentially the same conditions of comparison in the A-series as in the B-series. The average of the results for B1, B2 and B3 are equal respectively to the results of A1, A2 and A3. B4 shows a normal illusion of the vertical.

From the comparison of the results of these two series of tests, we conclude that the described illusion of length in surfaces is essentally a linear illusion.

Is this linear illusion contingent upon the difference in the direction of the two lines compared? In Case C both lines are placed in the same direction, and the illusion of the vertical is eliminated. All the results for lines lying in the same direction show that the illusion is present in such lines, but is very much diminished by the elimination of the difference in direction.

Case D was introduced in order to determine whether the illusion of distances in the same direction is contingent upon the presence of lines. The results are affirmative; there appears to be no illusion for the distance between the dots.

Case E was introduced in order to determine whether excessive eye movements constitute one of the motives for the illusion. In Cases A, B and C the comparison is naturally made by bisecting the long line visually, to determine whether one-half of the long line is equal to the length of the short line. When one-half of the line is regarded, the eye wanders beyond the middle point into the other half, on account of the guiding power of the line and the absence of a definite objective limit. This overrunning of the point of regard ought to produce an overestimation of the half that is regarded, and thus an overestimation of the whole line. Case E is like Case C in every respect except that there is

an additional motive for excessive eye movements in Case E; the eye is allowed to wander to the right beyond the limit to be selected. The results show that the introduction of this additional motive for excessive eye movements produces a marked increase in the illusion. Since this new eye-movement motive, which is present in the bisection of a line, is of the same nature as the additional one which increases the illusion in Case E, we conclude that it is one of the primary causes of the illusion of length. We may refer to this as the first motive.[1]

Contrast is a second primary motive which may be present and co-operate with the first motive. It is an application of the principle of relativity—a long line is compared with a short line, and, according to this principle, the long line should appear to be longer and the short line shorter than it really is. With young children this contrast is undoubtedly the strongest motive, because children have strong tendencies to overestimate differences. It has been demonstrated that illusions which have physiological causes, *e. g.*, the illusion of the vertical and the Müller-Lyer illusion, do not vary in a marked manner with mental development; but the illusions of judgment, *e. g.*, the illusions of contrast and illusions of time, vary very much with mental development.[2] Therefore it is probable that, if both the physiological and the psychical motives were present in those of the tests that were made upon children, the variation with age (mental development) is due chiefly to variation in the second motive, namely, contrast.

These two motives appear in the comparison of lines that

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[1] Incidentally we have here a crucial test of the eye-movement theory of the Müller-Lyer illusion. Cases C and E may be considered the simplest forms of the Müller-Lyer figure. All the motives demanded by current theories of this illusion, except one, are eliminated in these figures. The eye-movement motive is the only one present, and the illusion is present and varies with this motive.

[2] SEASHORE, *op. cit.*, pp. 33–35, 83, 84.

lie in the same plane and direction, but when the two lines lie in different directions or are parts of parallelograms, there appears in addition to these a third primary motive. It is a well-known fact that if a square and a double square appear side by side the latter will appear to be the narrower. This is not due to the contiguity of the two figures, for the dimensions of the double square do not appear to change when the square is removed.[1] Some would ascribe it to the principle of relativity, the two sides of the same figure being compared with each other. But it may be accounted for better by Wundt's theory of eye movements, *i. e.*, there is a stronger tendency to move the eyes in the direction of the longer lines than in the direction of the shorter.[2] This theory is in harmony with the most acceptable theory of the visual perception of space and is well supported by the fact that the total illusion of length is so much greater for parallelograms and lines at right angles to each other than for the lines that lie in the same plane and direction.[3] The results tend to show that this third primary motive, which has no connection with the first eye-movement motive, is, for adults, the strongest motive in the figures that contain it.

To determine the validity of the method employed, we made parallel measurements by the method of selection. The observers were allowed to select the required proportions from a series of cards. The results obtained by this

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[1] The theory that the apparent narrowing of the double square is due to an illusion of perspective is disproved by the fact that the figure does not appear to change when the square with which it has been compared is removed, and by the fact that the length of the figure is overestimated, instead of underestimated.

[2] Wundt, *Die geometrisch-optischen Täuschungen*, Abh. d. math.-phys. Cl. d. k. Sachs. Ges. d. Wiss., XXIV., 1899, 158.

[3] A part of this difference may be accounted for by the fact that the placing of the lines in different directions increases the difficulty of comparing and thus gives fuller sway to the motives for illusion that are present.

method support those obtained by the method of production as described above.

We also made a series of measurements by the method of selection to determine whether the ratio 2:1, employed above, is peculiarly favorable for the production of the illusion. The observers were required to estimate the length of cards in terms of their standard width. There appears to be a gradual increase in the illusion from its inception near the square up to the ratio 2½:1, which was the largest ratio tried. The illusion is not relatively stronger for the ratio 2:1 than for the adjacent ratios.

Does this illusion vary systematically with the size of the figures? The averages of the results for four observers who made eighty trials each upon each of four sizes of cards of the A2 type above, by the method of production, are as follows: standard (length in the vertical direction) 76 mm., illusion 7%; standard 114 mm., illusion 8%; standard 228 mm., illusion 7%; and standard 456 mm., illusion 8%. The cards were in all cases 75 cm. from the eyes. There is therefore no constant tendency for the combined illusions to vary with the size of the cards. But it has been held that the illusion of the vertical alone increases with the size of the object. We therefore measured the illusion of the vertical for the corresponding sizes of squares. The same four observers made eighty trials each for each size of card, by the method of production, with the following average results: standard 57 mm., illusion 3%; standard 114 mm., illusion 2%; standard 228 mm., illusion 4%; and standard 456 mm., illusion 3%. Thus there is no constant tendency for the illusion of the vertical to vary with the size of the object, and therefore the same is proved for the illusion of length.

NORMAL ILLUSIONS IN REPRESENTATIVE GEOMETRICAL FORMS

BY

MABEL CLARE WILLIAMS

The experiments here reported were made for the purpose of determining the chief motives for visual illusions in the form of common objects. The research was planned primarily with reference to the analysis of some of the illusions found in the cylinder, but its scope gradually widened and led to the study of several other fundamental geometrical forms. The aim of the experiments was somewhat broader than simply to verify or disprove a particular hypothesis. The natural-history method of collecting and classifying data is the predominating feature of the plan. The aim has been to gather a large number of data systematically, especially in the form of judgments of persons who were not aware of the purpose of the study and who were also often unaware of the existence of the illusions, thus employing the so-called unconscious method as far as possible. This method of procedure is especially well adapted for the study of normal illusions, and in fact is really necessary, because normal illusions change when attention is directed to them and knowledge of the illusions leads to unconscious corrections. The naive judgments give a true measurement of the force of the illusion and new illusions are most readily discovered by this method. It is the puzzling residuals that appear unexpectedly and for some time baffle all efforts at explanation that in the end furnish the most reliable and convincing demonstration of fact.

The observers may be classified with reference to three general types: first, adults having knowledge of the illusions but making no conscious allowance for them; second,

adults without knowledge of the illusions; and third, children without knowledge of the illusions. The degree of knowledge of the subject possessed by each observer was ascertained as nearly as possible and the records interpreted with reference to this knowledge. In computing the results, the method of the average value has been employed. In many instances the median value might be more representative but for the sake of uniformity the average value has been used.

Each of the chief factors which modify the illusions has been varied in turn: the size, distance, form, and position of the object; the different classes of observers with regard to age, sex and intelligence; the different subjective conditions, such as the degree of knowledge, attention, and practice; and the different methods of judging.

The order of sequence adopted in the study of the different forms is due to two factors: first, the natural evolution of the research and second, the aim to avoid in each series and in all the series as a whole the effect of suggestion.

In every series where more than one judgment upon a point was secured from each observer the so-called double fatigue order was employed; that is, half the number of trials on each point were made in going over the series the first time, and, upon returning in the reverse order, the other half of the trials were made.

The horizontal line was selected as the best form in terms of which to state the illusions in the other forms studied. The line in this position is the simplest and most fundamental form and it seems to be freer from illusions than any other form.

The 114 mm. standard was adopted partly to relate this research more closely to other experiments in the same laboratory and partly for the reason that in other tests this size had been found to be very satisfactory. Only the most typical or fundamental forms were studied, it being

assumed that what is true for these would also be true in some measure for their derivatives.

The forms which were studied are shown in Figure 1, and they are named and described in the following list.

EXPLANATION OF FIGURE 1.

1. The square plate. 114 mm. x 114 mm.
2. The cube. 114 mm. x 114 mm. x 114 mm.
3. The cylinder. Length, 114 mm.; diameter 114 mm.
4. The sphere. Diameter 114 mm.
5. The triangle. Base 114 mm.; altitude 114 mm.
6. The pyramid. Base 114 mm.; altitude 114 mm.
7. The cone. Base 114 mm.; altitude 114 mm.
8. The disk. Diameter 114 mm.
9. The triangle and plate. Base 114 mm.; total length 228 mm.
10. The pyramid and cube. Base 114 mm. square; total length 228 mm.
11. The cone and cylinder. Diameter of base 114 mm.; total length 228 mm.
12. The circle. Diameter 114 mm.
13. The drawn square. 114 mm. x 114 mm.
14. The ellipse. Long axis 114 mm.; short axis 38 mm.
15. The drawn cylinder. Length 114 mm.; diameter 114 mm.
16. The line. Length 114 mm.

Forms 1, 2, 3, 4, 5, 6, 7, 9, 10, and 11 were made of tin and painted a lustreless black. Forms 8 and 14 were cut from dull black paper and pasted upon a sheet of light manilla cardboard one meter square. Form 1 was either cut from the paper or made of tin. Forms 12, 13, 15, and 16 were drawn upon a sheet of cardboard, the limiting lines being one millimeter wide. The end of the cylinder, Form 15, was represented by an ellipse the axes of which were in the ratio 3:1.

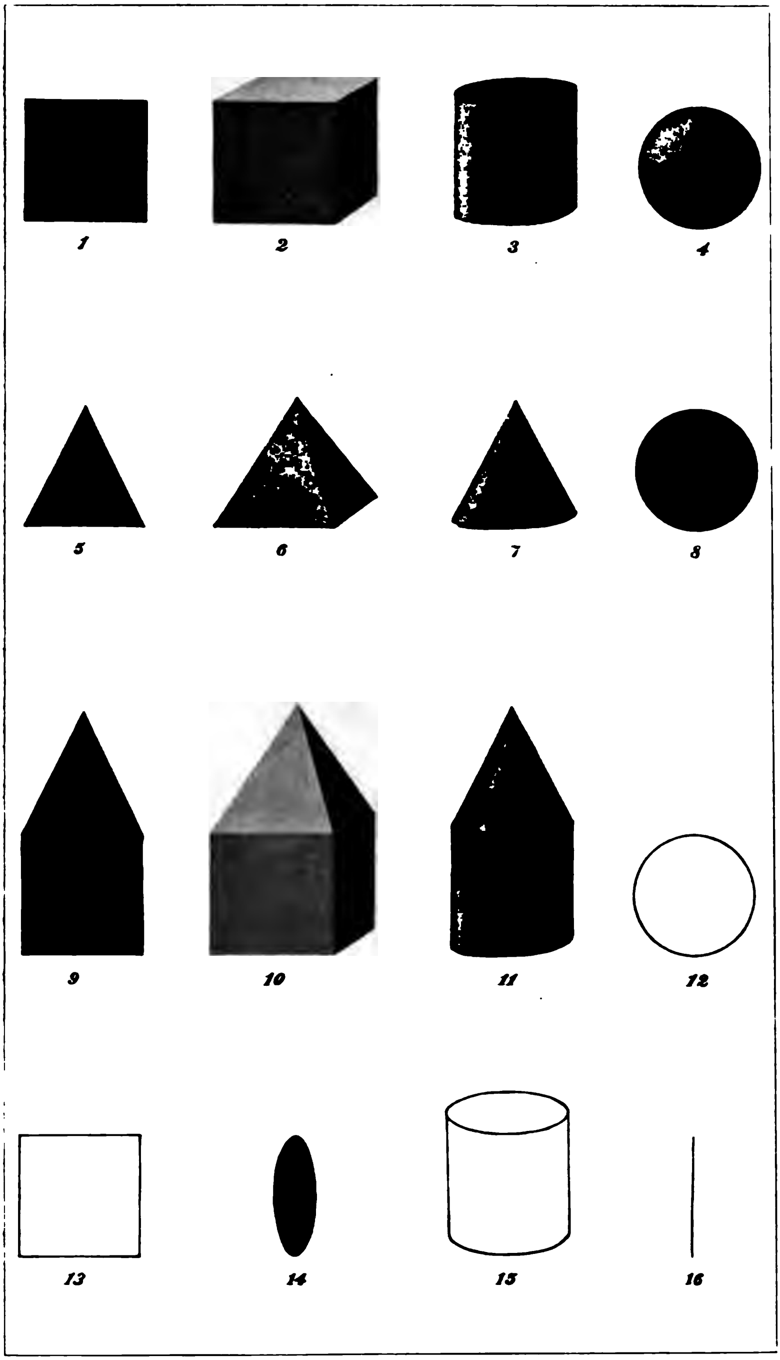

FIGURE 1

Series I

A preliminary study of the cylinder revealed a peculiar illusion which appears in the overestimation of its length. This will be called the illusion of cylinder length. The object of the experiments in Series I. was to collect measurements on this illusion in the length of the cylinder and the illusion of the vertical in the square. The experiments were made on school children in order to secure naive judgments and to discover how these illusions vary with age, sex and intelligence. They constituted one of nine systematic tests made upon one hundred and twelve pupils in the grammar school of Iowa City in the spring of 1900. The other eight tests were on different subjects, such as muscular fatigue, discriminative action, discrimination for the pitch of tones, hearing-ability, and keenness of vision. This combination of tests led the children to infer that their eyes were being tested and helped to keep them ignorant of the illusion. The experiments were made Saturday mornings. No results of this test were given out to the children.

Three measurements were made upon the cylinder and one upon the square, as follows:

Case 1. The vertical cylinder[1] at the level of the eyes: to select the one whose diameter and length are equal.

Case 2. The plate, at right angles to the line of vision: to select the one whose height and width are equal.

Case 3. The horizontal cylinder, at the level of the eyes: to select the one whose diameter and length are equal.

Case 4. The vertical cylinder, standing thirty degrees below the level of the eyes: to select the one whose diameter and length are equal.

[1] The term "vertical cylinder" denotes a cylinder with the length in the vertical direction, and the term "horizontal cylinder" one whose length is in the horizontal direction.

The apparatus for Cases 1, 3, and 4 consisted of a series of fifteen cylinders (Fig. 1, Form 3) from which the observer was required to select the one in which the length appeared to be equal to the diameter. They were each 114 mm. in diameter but varied in length, by five-millimeter steps, from 69 mm. to 134 mm. For Case 2, the apparatus consisted of fifteen plates (Fig. 1, Form 1) each 114 mm. wide but varying in height, like the length of the cylinders, from 69 mm. to 134 mm. Both the cylinders and the plates were made of tin and were painted dull black. They were kept out of view in a case and the experimenter exhibited one at a time by placing it upon the center of a stand which was so adjusted, except in Case 4, that the middle of the form was at the level of the eyes of the observer. The stand was one foot square and was covered with gray cloth. The observer was at a distance of one and one-half meters.

The method of selection employed was the following: The cylinder whose length was equal to its diameter was presented first and the observer stated whether the length appeared to be equal to the diameter. If he said it was too long, the next shorter cylinder was presented and after that other short ones in order until the "equal" point had been passed and two consecutive cylinders had been pronounced too short; then the cylinders were presented in the opposite order until the "equal" point had been passed again and two consecutive cylinders had been pronounced too long. If, on the other hand, the observer said that the first cylinder presented was too short, the above order was reversed. Thus the judgments of the observers determined how many cylinders should be presented, and in what order, and the upper and the lower limits of the region of equality were determined twice for each observer. The procedure may be represented in the following scheme, in which the cylinders are denoted by the numbers indicating the length of the cylinder and the judgments upon

each by the letters. L denotes the judgment "too long"; E, "equal"; and S, "too short."

99	104	109	114	119	124
S	S	E	L	L	
	S	E	E	L	L

Here are two complete determinations; the first is 109 and the second is 111½. The mean between these (110, neglecting fractions) is taken as the final record. Similar evaluations were made for other sequences of judgments. The same method was employed in the selection of the square.

The results are given in Tables I*A* and I*B*, which also contain the classification of the children according to age, sex, grade, school standing, and teacher's estimate of mental ability.

In Case 1 the average illusion for the boys is 17.5% and for the girls, 18.4%. This means that if the length of a vertical cylinder is to appear to be equal to the diameter of the same, the length must be about 18% shorter than the diameter. To the average boy the length of the vertical cylinder appears to be equal to the diameter when the length is 94 mm. and the diameter 114 mm., and boys vary from each other in this judgment by an average of only 4.4%. In this overestimation of the height of a cylinder, the illusion of the vertical and the illusion of cylinder length coöperate. But, when the cylinder is laid on its side, as in Case 3, they conflict and the illusion of the vertical causes an overestimation of the diameter while the illusion of cylinder length causes an overestimation of the length in a horizontal direction. If the two illusions were of equal strength, their effects would cancel each other, but the table shows that for these children the illusion of the vertical is on the average 1.8% stronger than the illusion of cylinder length.

In Case 2 the illusion of the vertical in the square was

TABLE I. (*A*). *Boys' Records.*

Obs	Age	Gr	St	Ab	Case 1 E	Case 1 d	Case 2 E	Case 2 d	Case 3 E	Case 3 d	Case 4 E	Case 4 d
2	16	8	B	B	93	4	102	3	122	0	90	4
4	13	8	D	B	101		100	1	97		109	3
6	15	8	D	C	94	3	98	1	125	4	95	4
8	14	8	D	C	95	4	100	5	115	1	88	7
10	16	8	E	E	93	4	109	0	103	1	102	3
12	14	8	E	D	95	4	104	0	120	2	83	7
14	13	8	A	A	95	2	109	0	112	2	98	4
16	15	8	A	A	103	4	108	1	119	0	100	2
18	14	8	B	C	93	1	100	3	113	7	90	4
20	13	8	C	B	85	1	99	0	127	3	87	3
22	12	8	C	B	93	4	105	1	115	2	99	3
24	14	8	A	B	89	3	104	0	109	0	99	
26	13	7	C	B	97	3	103	1	124	0	97	3
28	12	8	C	C	95	1	107	3	103	1	91	3
30	13	8	E	D	79	5	98	4	122	3	94	
32	12	8	C	B	104	3	110	1	109	0	107	3
34	13	8	D	C	85	1	99	3	122	3	84	3
38	15	8	C	C	84	3	99	0	122	0	85	4
40		8	D	C	84	0	108	1	118	1	89	
42	14	8	D	C	95	2	105	2	108	1	97	3
44	13	8	C	B	92	3	106	3	121	2	97	3
46	13	8	C	C	87	5	100	2	120	7	88	4
48	14	8	A	A	96	0	102	0	120	2	95	2
50	15	8	E	D	99	2	99	2	109	2	94	5
52	12	8	D	C	87	5	100	3	105	2	90	2
54	12	8	B	A	99	2	99	2	112	2	100	2
56	12	8	B	A	99	0	102	0	114	0	95	2
58	13	8	A	B	98	1	104	0	113	1	92	3
60	15	8	D	C	94	3	107	3	110	2	93	1
62	14	8	D	C	90	2	105	2	107	3	89	3
64	13	8	D	C	109	3	103	4	120	3	109	3
66	12	7	C	C	92	3	100	2	112	0	88	1
68	13	7	D	C	95	4	100	2	113	4	98	3
70	15	8	C	C	104	3	102	0	114	3	99	3
72	12	8	C	C	94	3	107	0	107	0	92	0
74	14	7	D	D	98	4	105	1	109	0	84	5
76	16	7	D	D	89	3	107	3	104	3	90	4
78	14	7	C	C	80	6	97	0	123	1	82	8
80	13	8	A	A	98	1	107	3	128	1	97	3
82	14	8	A	A	98	1	109	0	114	0	99	3
84	12	7	D	C	89	5	97	3	124	0	94	0
86	12	7	C	B	99	5	107	3	125	4	97	3

TABLE I. (*A*). *Boys' Records*. Continued.

					Case 1		*Case 2*		*Case 3*		*Case 4*	
Obs	*Age*	*Gr*	*St*	*Ab*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
88	13	7	B	B	90	4	102	5	123	1	99	3
90	12	7	A	A	97	5	103	4	117	3	98	4
92	12	7	A	A	99	3	103	1	124	0	98	1
94	14	7	C	C	99	5	107	3	117	3	104	0
96	13	7	C	C	107	3	108	1	119	0	108	1
98	11	7	A	A	97	3	104	3	125	5	98	4
100	15	7	C	B	103	3	107	3	112	0	99	0
102	12	7	A	A	99	2	104	2	122	9	95	4
104	12	8	A	A	86	3	104	0	125	2	93	5
106	13	8	B	A	98	1	104	0	116	0	93	4
108	13	8	D	C	98	4	100	4	122	3	88	6
110	14	7	C	C	94	3	105	2	110	1	97	3
112	15	7	D	D	94	3	104	3	110	3	94	2
116	13	7	A	A	97	0	104	2	122	0	98	5
118	15	7	C	C	92	0	104	0	114	0	88	4
120	12	7	B	B	88	4	103	1	125	4	88	4
			Average		94	3	103	2	116	2	95	3
			Il		—20		—11		+2		—19	
			%Il		—17.5		— 9.7		+1.8		—16.7	
			%D		4.4		2.6		5.4		4.4	

Obs., the observers, by assigned numbers—boys even, and girls odd numbers.

Age, age at nearest birthday.

Gr., grade in the public school.

St., average standing according to the last monthly teacher's report.

Ab., mental ability as estimated by the teacher—five grades, A, B, C, D, and E in order, A being the highest.

E, the recorded estimate. This is the mean between two complete determinations, given in millimeters.

d, the mean variation, *i. e.*, the average deviation of the result for each trial from the average result for all the trials of the same observer, regardless of sign.

Il., the amount of the illusion, in millimeters.

%Il., the amount of the illusion, in percent of the standard distance (114 mm.)

%D., the average percent of deviation of the individual observers from the average for the group.

The minus sign signifies *below*, and the plus sign *above* the standard.

TABLE I. (*B*). *Girls' Records.*

					Case 1		*Case 2*		*Case 3*		*Case 4*	
Obs	*Age*	*Gr*	*St*	*Ab*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	13	8	C	B	89	3	100	1	118	2	85	4
15	15	8	D	D	97		107		114		97	
21	14	8	D	D	97	0	105	2	107	3	96	4
23	13	8	C	B	100	2	107	3	120	2	95	4
25	15	8	C	C	97	3	104	3	112	3	87	5
27	13	8	C	B	85	2	97	0	117	3	89	5
29	13	8	D	C	100	3	108	1	109	2	100	2
31	13	8	D	D	82	8	105	2	115	4	89	7
33	14	8	D	D	84	3	88	1	119	3	95	4
35	15	8	A	B	93	1	100	4	112	5	93	4
37	14	8	E	E	93	4	105	2	109	0	97	3
39	14	8	E	D	84	5	104	3	94	5	74	3
41	14	8	C	C	77	3	97	5	103	2	81	0
43	16	8	D	D	83	9	98	4	117	3	80	6
47	14	8	C	C	95	2	104	0	107	0	89	3
51	13	8	C	C	97	3	105	2	112	0	95	2
53	14	8	A	A	89	3	105	2	118	2	92	0
55	15	8	C	C	98	1	104	3	118	1	94	3
57	13	8	D	C	94	5	104	3	109	3	89	3
59	13	8	B	B	104	3	109	0	118	1	102	3
61	13	8	B	C	95	4	102	3	118	1	93	4
63	12	8	A	A	95	7	105	2	119	0	94	3
65	14	8	A	A	88	4	103	1	114	0	86	2
67	14	7	D	E	109	3	109	3	118	1	107	0
69	11	7	E	E	102	0	99	0	120	1	94	5
71	14	7	D	C	95	5	102	3	118	1	90	2
73	12	7	C	B	89	3	108	1	123	4	95	4
75	11	7	D	D	96	5	99	3	109	0	90	4
77	13	7	D	D	104	5	110	2	105	2	88	5
79	11	7	A	A	85	2	99	0	115	2	105	4
81	14	7	B	B	85	6	108	1	123	1	94	5
83	16	7	D	D	92	3	104	3	118	3	92	3
85	13	7	C	C	95	2	107	3	124	0	87	8
87	12	7	A	A	99	3	104	3	114	3	94	3
89	11	7	A	A	99	2	108	1	124	3	97	3
91	13	7	B	B	98	1	103	1	122	5	98	1
93	12	7	D	D	90	4	99	3	120	2	89	5
95	13	7	B	B	85	4	99	0	124	0	95	5
99	12	7	C	C	89	5	98	1	119	3	94	5
101	13	7	C	C	90	4	107	3	120	3	94	3
103	11	6	D	C	97	3	105	2	124	3	98	5
107	14	7	D	E	92	4	103	4	114	3	90	4

TABLE I. (*B*). *Girls' Records.* Continued.

Obs	*Age*	*Gr*	*St*	*Ab*	*Case 1* *E*	*d*	*Case 2* *E*	*d*	*Case 3* *E*	*d*	*Case 4* *E*	*d*
109	14	7	C	C	88	1	104	3	121	1	90	1
113	12	7	A	A	90	6	102	3	113	1	91	5
115	12	7	A	A	90	6	97	3	122	0	90	4
117	16	8	B	B	94	3	104	3	112	0	94	0
119	13	7	A	A	99	5	105	2	122	0	104	0
121	11	7	B	B	105	4	99	2	119	2	94	5
123	14	8	D	C	93	4	104	2	125	4	98	5
125	13	7	C	C	95	6	104	2	114	0	103	1
129	13	7	D	C	93	1	107	3	109	0	98	4
131	13	7	B	B	85	4	104	0	114	0	94	5
135	15	7	B	B	94	0	108	1	112	0	95	1
139	13	8	B	B	100	4	97	3	132		102	
			Average		93	4	103	2	116	2	93	3
			Il		—21		—11		+2		—21	
			%Il		—18.4		— 9.7		+1.8		—18.4	
			%D		4.4		2.6		4.4		4.4	

measured. It amounts to 9.7% both for boys and for girls, and the children vary from one another in this judgment by an average of only 2.6%. It may be assumed for the present that the illusion of the vertical which enters into the overestimation of the height of the cylinder is the same as that for the square, the conditions being similar and the dimensions equal. If then, in the present method of comparison, the overestimation of the height of the cylinder is caused by two illusions only, the illusion of cylinder length amounts to 8.3%, which is found by subtracting 9.7 from 18. Again, if the illusion of the vertical in Case 3 is 9.7% for the diameter and the illusion of the vertical exceeds the illusion of cylinder length by 1.8%, then the latter amounts to 7.9%. There is thus less than 1% difference in the results for these two independent determinations of the illusion of cylinder length. The records demonstrate that the illusion of cylinder length exists independently of the illusion of

the vertical and is nearly equal to this familiar illusion, from which it has not to the writer's knowledge before been distinguished.

It was supposed that the cylinder would look longer when an end was in view, as it is shown in Fig. 1, Form 3, than when neither end could be seen, as in Cases 1 and 3. But the records for Case 4 reveal no appreciable difference. That is, the illusion of cylinder length is practically the same in the three fundamental positions in which the length of the cylinder may be seen.

There is no significant difference between the records of the boys and the records of the girls. The illusion is of about equal force, the mean variations are about equal, and the variation among the individuals is about the same for the boys and the girls.

In order to determine the variation of the illusion with age, these children may be taken as a group to represent children who are old enough to be intelligent observers and still are not mature mentally. They will be compared with adults in the next series.

To determine whether or not these illusions vary with mental ability, the records are classified, first, according to class standing (Table II) and, second, according to "mental ability" (Table III). The standing represents the averages from the last monthly reports. These are divided into five grades, represented by the letters in order, A being the highest. Mental ability was estimated by the teacher who knew the children best, and is equivalent to the dividing of the children into the five groups, "very bright," "bright," "average," "dull," "very dull."

The illusion of the vertical in the square does not vary with intelligence (See Case 2 in each table).

The illusion of cylinder length shows a tendency to be stronger for the less intelligent. This conclusion rests chiefly upon Case 3, for which both tables show that, in the conflict of the two illusions involved, the illusion of the

TABLE II.

		Case 1			Case 2			Case 3			Case 4		
St	*n*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*
A	23	—16.7	3	3.5	—8.7	2	1.8	+3.5	2	4.0	—15.8	3	2.6
B	17	—17.5	3	4.4	—9.6	1	2.0	+4.4	1	4.4	—16.7	3	2.6
C	32	—18.4	3	4.4	—9.6	2	2.6	+3.5	3	4.4	—17.5	3	5.2
D	33	—15.0	4	5.2	—9.6	3	2.6	0	2	5.2	—18.4	4	4.4
E	7	—19.3	3	5.2	—9.6	2	2.6	—2.6	2	7.0	—20.2	4	6.2

n, the number of children in each group.
Other notation in this and the next table, same as in Table 1.

TABLE III.

		Case 1			Case 2			Case 3			Case 4		
Ab	*n*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*	*%Il*	*d*	*%D*
A	23	—15.8	3	3.5	—8.7	2	2.0	+4.4	2	3.5	—15.8	3	3.0
B	27	—17.5	3	5.0	—8.7	2	3.0	+3.5	2	5.2	—15.8	3	3.5
C	41	—18.4	3	4.4	—9.6	2	2.6	+1.0	2	5.2	—18.4	3	3.5
D	16	—19.3	4	5.2	—10.5	3	2.6	—1.8	2	5.2	—21.9	5	4.0
E	5	—14.0	3	5.2	—8.0	2	2.6	—1.0	1	5.2	—14.0	3	4.4

vertical is stronger than the illusion of cylinder length for the very bright children, and the illusion of cylinder length is stronger than the illusion of the vertical for the very dull. This is also clearly corroborated in Cases 1 and 4 in both tables, for there is a variation in the combined illusion but the illusion of the vertical does not vary, as was shown in Case 2.

The figures in the %D columns show that the more intelligent children are in closer agreement with one another than the less intelligent. In other words, the illusion is more uniform for the bright than for the dull children.

The conclusions for this first series of experiments, upon the children, may be summarized as follows:

The illusion of the vertical for the square is about 10%.

The illusion of cylinder length is about 8%.

The illusion of cylinder length does not vary with the position of the cylinder.

Both the illusion of the vertical and the illusion of cylinder length are of approximately the same force for boys and girls.

The illusion of the vertical for the square does not vary with intelligence.

The illusion of cylinder length varies with intelligence.

The illusions are more constant for the bright than for the dull children.

Series II

The purpose of this series was the further development of some of the problems presented in Series I. The observers, three women and eight men, were members of the University Summer Session of 1900. They were mature students, being teachers in the public schools of the state, but their knowledge of illusions was very vague and imperfect. In general the observers knew something of the illusion of the vertical but they knew nothing in regard to the other illusions which are under consideration.[1] They were given no suggestion of the nature of the problem, either before the experimenting began or during its progress.

The study of the illusions in the plate and the cylinder was continued by repeating the four cases of Series I. The plate was also compared with lines and the cylinder with lines and surfaces. The same plates and cylinders were used as in the previous series. Twelve cases were introduced into the series as follows:

Cases 1 to 4. The same as in Series I.

Case 5. The vertical cylinder: to select a plate whose width is equal to the diameter of the cylinder.

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[1] With regard to the illusion of the vertical, the observers belong mainly to type one, as defined on p. 38, but with regard to the illusion of cylinder length and other then unknown illusions, they belong to type two.

Case 6. The plate: to mark off a vertical distance equal to the height of the plate.

Case 7. The vertical cylinder: to select a plate whose height is equal to the length of the cylinder.

Case 8. The vertical cylinder: to mark off a vertical distance equal to the length of the cylinder.

Case 9. The vertical cylinder: to select a vertical line equal to the length of the cylinder.

Case 10. The plate: to select a vertical line equal to the height of the plate.

Case 11. Vertical cylinders: to select from a series of cylinders those whose heights are equal respectively to one-half, one, two, and four times the diameter.

Case 12. Rectangular plates: to select from a series of plates those whose heights are equal respectively to one-half, one, two, and four times the width.

In Cases 1 to 4 the same method was used as in Series I. The same method was also employed in Cases 5, 7, 9, and 10, except that the dimensions of two different objects instead of two dimensions of the same object were compared. For these four cases two large backgrounds, covered with seamless gray cloth, stood at right angles to each other; the standard form was placed in the center of one of these and the form to be compared with it in the center of the other. For Cases 9 and 10, a series of fifteen wires, 2.5 mm. in diameter and varying in length from 69 to 134 mm., served as lines. A small hole, scarcely visible at the observer's distance, was drilled near one end and the wires were hung upon a projecting pin as needed.

In Cases 6 and 8 a method of production was used. Two tables were placed at right angles to each other and had erected upon them backgrounds of the same dimensions as the tops of the tables. Both tables and backgrounds were covered with gray cloth. The standard form was placed upon one table against the middle of the background and the observer indicated upon the other background with a

pointer one meter long, a distance above the table which he judged to be equal to the height of the standard form.

The apparatus for Case 11 was a series of twenty-three wooden cylinders painted black, each having a diameter of 114 mm. The lengths were 36, 40, 45, 52, 57, 64, 71, 80, 90, 101, 114, 128, 143, 163, 182, 223, 228, 256, 288, 324, 365, 407, 456 mm. These cylinders were placed upon a table in the order of size and about 50 mm. apart, and the observers simply pointed to the cylinders they selected.

In Case 12 rectangular black cardboard plates, 114 mm. wide and corresponding in height to the lengths of the wooden cylinders, were used. They were fastened invisibly to a long strip of gray cloth, 90 cm. wide, which was tacked upon the wall.

In addition to the measurements upon the forms 114 mm. in length, measurements were also made upon the forms 104 mm. and 124 mm. in length for Cases 6 to 10 inclusive. The purpose of the tests upon these forms was to secure a check upon the measurements on the regular 114 mm. standards and also to eliminate any suggestion which might rest upon the employment of forms that have equal dimensions.

All the forms were on a level with the observer's eyes, unless otherwise noted, and one and one-half meters away. Four determinations were made in each of the first ten cases and only one determination in each of the last two cases.

The averages of all the estimates of the observers, the average individual mean variations, and the percentage of illusion for the various cases are presented in Table IV. In Table V the individual estimates are given for the 114 mm. square and 114 mm. cylinder. This table is introduced to show the extent of agreement of the different observers with one another.

TABLE IV.

	Standard 104 mm.			Standard 114 mm.			Standard 124 mm.				
Case	*E*	*d*	*%Il*	*E*	*d*	*%Il*	*E*	*d*	*%Il*	*E*	*%Il*
1				100	3	—12					
2				107	2	— 6					
3				109	2	— 3.5					
4				97	3	—15					
5				123	3	+ 8					
6	108	3	+ 3.8	116	3	+ 1.8	126	3	+ 1.6		
7	121	3	+16	130	2	+14	135	1	+ 9		
8	116	3	+12	127	3	+11	136	4	+ 9		
9	114	3	+10	125	3	+10	134	1	+ 8		
10	106	2	+ 2	115	2	+ 0.9	125	2	+ 0.8		

	Standard 57 mm.		Standard 114 mm.		Standard 228 mm.		Standard 456 mm.	
11	54	— 5	93	—18	204	—11	366	—20
12	55	— 3	112	— 1.8	211	— 7	383	—16

The notation is the same as in Table I.

TABLE V.

Case	*1*		*2*		*3*		*4*		*5*		*6*		*7*		*8*		*9*		*10*		*11*	*12*
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*E*
2	99	3	107	3	114	0	102	3	115	4	113	0	122	0	129	3	105	2	103	3	91	102
4	103	2	105	2	108	1	96	3	119	0	120	3	132	5	128	4	129	5	107	4	91	114
1	103	4	107	0	109	0	94	0	131	1	125	5	134	2	138	7	134	2	113	3	102	114
6	105	3	107	3	105	2	103	4	120	6	112	5	130	2	117	3	125	3	120	3	102	114
8	101	2	110	2	110	3	96	4	125	2	117	2	134	0	126	4	132	2	124	1	91	114
10	94	5	109	0	114	1	99	3	117	1	108	3	125	2	117	3	123	3	111	2	91	114
5	97	3	107	0	108	1	92	3	121	3	115	4	124	5	123	2	127	3	112	1	81	102
3	94	0	102	0	110	2	89	3	123	1	127	5	130	2	138	2	118	1	115	1	91	114
12	102	3	106	3	98	4	91	2	134	5	112	4	135	3	122	3	133	2	122	3	81	114
14	102	5	109	2	109	3	107	3	122	3	109	3	126	2	121	3	127	3	123	2	102	114
16	97	2	111	2	110	4	94	1	125	1	121	3	132	1	133	3	125	2	116	2	102	114
Ave	100	3	107	2	109	2	97	3	123	3	116	3	130	2	127	3	125	3	115	2	93	112
Il	—14		— 7		— 5		—17		+ 9		+ 2		+16		+13		+11		+ 1		—21	— 2
%Il	—12		— 6		— 3.5		—15		+ 8		+ 1.8		+14		+11		+10		+0.9		—18	— 1.8
%D	2.6		1.8		2.6		4		4		4.4		3		5.2		4.6		4.6		5	3

The notation is the same as in Table I. In the first column the even numbers refer to the men and the odd numbers to the women. The individual records are presented in the order in which they were made.

The plate was studied in Cases 2, 6, 10, and 12. From Case 2 there is obtained a measurement of the illusion of the vertical. This is shown in the selection of a plate whose height is 6% less than the width; that is, a plate 107 mm. tall and 114 mm. wide was judged to be square. The result for the same experiment upon the school children was —9.7% (Series I, Case 2). In Case 6, where the height of the plate was marked off with a pointer, and in Case 10, where the line equal to the height of the plate was selected, the illusion of the vertical appears in both the standard and compared forms and is therefore fairly eliminated. In Case 6 there is a residual of +1.8% and in Case 10 of +0.9%. These residuals are due to other illusions which will be discussed in a subsequent series. For the 114 mm. standard in Case 12, the illusion of the vertical is smaller than usual. This is probably due to the use of a long series of parallelograms, and also to the fact that the observers reacted against the illusion of the vertical.

The tests upon standards 57, 228, and 456 of Case 12 are interesting, since they represent the balancing of two illusions. With these forms there is in addition to the illusion of the vertical, the illusion of length, according to which the longer dimension of a parallelogram is overestimated.[1]

When the form is longer in the horizontal dimension, as with standard 57, the illusion of the vertical and the illusion of length conflict, the former tending to cause the selection of a plate whose height is too small and the latter one whose height is too great. The result is —3%; that is, the illusion of the vertical is stronger than the illusion of length by 3% in this instance. When the height of the form is two and four times the width (Standards 228 and 456 respectively), the illusion of the vertical coöperates with the illusion of length, the effect of each being to cause

[1] See SEASHORE and WILLIAMS, *An Illusion of Length*, Psychol. Rev., 1900, VII., 592. Reprinted in this volume, p. 29.

the selection of a plate which is too short vertically. The forms actually selected were too short by 7% and 16% respectively. The sum of the two illusions apparently increases with the length of the form. In these experiments there is no way of determining whether this variation is in one or both of the illusions.

The illusion for the vertical cylinder is 12% for Case 1; that is, the length and diameter of a cylinder appear equal when the length is 12% less than the diameter. The child observers selected a cylinder which was 18% too short. (Series I, Case 1). These amounts represent the coöperation of the illusion of the vertical and the illusion of cylinder length. The former illusion is 6% for the plate and it may be assumed that it is the same for the length of the cylinder. The illusion of cylinder length then amounts to 6%.

It was stated in the discussion of Series I that the illusion of the vertical and the illusion of cylinder length conflict when the cylinder is laid upon its side as in Case 3. The result for this case is —3.5%; that is, the illusion of cylinder length outweighs by 3.5% the illusion of the vertical. If the illusion of the vertical is 6%, then the illusion of cylinder length is 6% plus 3.5% or 9.5%. In order to determine the variation of the illusion of cylinder length with the age of the observers, these two statements of the illusion for adults, 6% and 9.5%, should be compared with the corresponding statements of the illusion for children, 8.3% and 7.9%, as shown in Series I.

In both Case 4 and Case 1, the length of the vertical cylinder was compared with the diameter but the observers occupied different positions with reference to the cylinder. In Case 4 they looked down upon it at an angle of thirty degrees. The illusion is 3% greater for Case 4 than for Case 1. This is probably due to an underestimation of the diameter on account of the Müller-Lyer effect. The illusions for the cylinder are of greater force in Case 11,

standard 114, than in Case 4. With the other three standard cylinders of Case 11, the illusion of length, the illusion of the vertical, and the illusion of cylinder length enter. In general these illusions, taken together, seem to increase with the increase in the length of the cylinder.

In the discussion of the cylinder up to this point, only the ratio of the length and diameter has been considered. In Case 5 the diameter of the cylinder was compared with plates with the result that a plate 8% wider than the horizontal diameter of the cylinder was selected as equal to it. This means that there is an overestimation in the diameter (width) of the cylinder which does not appear in the width of the plate. For the present this illusion will be called the illusion of cylinder diameter but it will be explained and renamed in the following series of experiments. Now, it is evident that if the diameter of the cylinder is overestimated, the results in those cases in which the length and diameter of the cylinder are compared do not represent the full force of the illusions involved. For instance, the illusion of length in the cylinder in Case 1 is of sufficient strength to exceed by 6% the illusion of cylinder diameter, which is 8%. The illusion of cylinder diameter would cause too long a cylinder to be selected but, eliminating the illusion of the vertical, a cylinder 6% too short is selected. The illusion in the length of the cylinder, exclusive of the illusion of the vertical, then, really amounts to 14% in Case 1. It will be observed that the illusion of cylinder length is only a part of the illusion in the length of the cylinder. A further analysis of this fact will be given in the next series. In Case 7 the height of the plate was compared with the length of the cylinder. By this method the illusion in the length of the cylinder is found to be 14%. The results obtained by these two radically different methods are the same. Two more independent statements of the illusion in the length of the cylinder are obtained from Cases 8 and 9. These are 11% and 10%

respectively for the two cases. These figures would be larger if the methods used did not involve so many complications. In Case 8 the presence of the base line, and in Case 9 the presence, in the wire, of a new illusion to be discussed later, reduces the force of the illusions.

The illusion of cylinder length may be estimated to be about 8%, and, taking all the above data into consideration, the total illusion in the length of the cylinder, exclusive of the illusion of the vertical, is 14%. There is therefore a residual of about 6% in the length of the cylinder.

All the essential features found in the experiments with the 114 standard (the form in which the two dimensions are equal) are confirmed by the results for the experiments upon the two forms in which the dimensions were not equal (Standards 104 and 124 of Cases 6 to 10 inclusive). The difference in the proportions of the two dimensions of the form has however a marked effect, as may be seen in Table IV.

The general conclusions drawn from the results of this series of experiments, under the conditions described, may be summarized as follows:

The illusion of the vertical in the square is about 6% for this type of observers.

The illusion of the vertical in the square is not so strong for the adults as for the children. (Series II, Case 2; Series I, Case 2).

The illusion of cylinder length is apparently as strong for adults as for children.

A new illusion appears in the overestimation of the height of the plate. (Cases 6 and 10).

The total effect of the illusion of the vertical and the illusion of length increases with the length of the parallelogram. (Case 12).

The total effect of the illusion of the vertical, the illusion of cylinder length, and the illusion of length increases with the length of the cylinder. (Case 11).

The illusion of cylinder length, as determined in two different ways, is found to be stronger than the illusion of the vertical.

The diameter of the cylinder is overestimated by about 8%. (Case 5).

There is a new illusion in the length of the cylinder which appears in an overestimation of about 6%.

The difference in the proportions of the two dimensions of an object influences the perception of the magnitude of one dimension. (Standards 104 and 124 of Cases 6 to 10 inclusive).

Series III

The experiments in Series III were made in the fall of 1900. The purpose of this series was, first, to establish further the illusions in the cylinder and square, which were brought out in the previous series, by studying them under more varied conditions and with different observers; and, second, to obtain data upon two new forms, the cube and drawn cylinder (Fig. 1, Forms 2 and 15).

The standard cube and cylinder were placed upon a small support in the center of a background of light manilla cardboard 1 meter square. The plate was cut from black paper and pasted at the center of a sheet of cardboard and the drawn cylinder was outlined at the center of another sheet. These backgrounds were of the same color and size as the one upon which the standard forms were placed.

The forms used for comparison with the standard forms were lines and plates (Fig. 1, Forms 16 and 1). A series of twelve black lines, 1 mm. in width and varying in length by 5 mm. steps, from 94 mm. to 149 mm., were drawn upon each of four manilla cardboard sheets, 1 meter square. The lines were drawn in no definite order with regard to length, but they were 50 mm. apart and parallel.

The arrangement of the lines was different in the four series.

There were four series of twelve rectangular plates each. These plates were all 114 mm. wide, but the lengths varied by 5 mm. increments from 94 mm. to 149 mm. in each series. They were fastened upon a background 2 meters square, covered with the light manilla cardboard. The four series were placed in as many rows; in the first the smaller plates were in the center and the larger ones at the ends and they varied in the vertical dimension; in the second row the smaller plates were also in the central part but the horizontal dimension varied; in the third row the larger plates occupied the central position and their vertical dimensions varied; and in the fourth row the larger plates were in the center and their horizontal dimensions varied. The plates were about 45 mm. apart in the row and the rows about 30 cm. apart. The height of this large background was adjustable and it was so arranged that the particular row from which the plate was to be selected was on a level with the eyes of the observer. By having several series of plates and lines the tendency of the observers to select from memory was obviated.

The background for the standard forms and that for the compared forms were placed at right angles to each other. The observer was seated upon a revolving office stool at a distance of 1 meter from the objects observed and was required to turn upon the stool so that the whole body moved through an angle of ninety degrees. The standard form was first placed before the observer, then a series of lines or plates with which the standard was to be compared was shown. The observer indicated with a long pointer the compared form which he selected.

Measurements were also made upon the cube, the cylinder, and the plate when these forms were so placed that the observer looked down upon them at an angle of thirty degrees. In the case of the cube and cylinder this afforded a view of the top. The forms with which the standard

TABLE VI.

Case	Dimension of Standard Form Measured	Compared Form	Direction	No. in Fig. 1
1	Height of cube	Lines	Vertical	2
2	Width of cube	Lines	Horizontal	2
3	Length of cylinder	Lines	Vertical	3
4	Diameter of cylinder	Lines	Horizontal	3
5	Height of cylinder	Plates	Vertical	3
6	Diameter of cylinder	Plates	Horizontal	3
7	Height of cube	Plates	Vertical	2
8	Width of cube	Plates	Horizontal	2
9	Length of cylinder	Plates	Horizontal	3
10	Diameter of cylinder	Plates	Vertical	3
11	Length of cylinder	Lines	Horizontal	3
12	Diameter of cylinder	Lines	Vertical	3
13	Length of drawn cylinder	Lines	Vertical	15
14	Diameter of drawn cylinder	Lines	Horizontal	15
15	Length of drawn cylinder	Plates	Vertical	15
16	Diameter of drawn cylinder	Plates	Horizontal	15
17	Length of drawn cylinder	Lines	Horizontal	15
18	Diameter of drawn cylinder	Lines	Vertical	15
19	Length of drawn cylinder	Plates	Horizontal	15
20	Diameter of drawn cylinder	Plates	Vertical	15
21	Length of cylinder*	Lines	Vertical	3
22	Length of cylinder*	Plates	Vertical	3
23	Height of cube*	Lines	Vertical	2
24	Height of cube*	Plates	Vertical	2
25	Height of plate	Plates	Vertical	1
26	Height of plate	Lines	Vertical	1
27	Height of plate*	Plates	Vertical	1
28	Height of plate*	Lines	Vertical	1
29	Diameter of cylinder*	Lines	Horizontal	3
30	Diameter of cylinder*	Plates	Horizontal	3

* The observer looked down upon these forms at an angle of 30 degrees.

forms were compared when in this position were on a level with the observer's eyes.

Except as just noted, only one side of the cube was visible and neither end of the cylinder. Eight trials were made on each case; the experiment lasted one hour a day for each observer and four days were required for some of the observers. The order of sequence of the cases was deter-

TABLE VII.

Case	*A*	*Vl*	*C-L*	*M-L*	*% Il*
1*	+	+	0	0	+12
2*	+	+	0	0	+14
3*	+	+	+	0	+15.7
4*	+	+	0	0	+12
5†	0	+	+	0	+ 9
6	0	+	0	0	+ 6.1
7†	0	+	0	0	+ 3.7
8	0	+	0	0	+ 5.2
9	0	+	+	0	+12.3
10†	0	+	0	0	0
11*	+	+	+	0	+18.5
12*	+	+	0	0	+10.4
13*	+	+	+	0	+17.5
14*	+	+	0	0	+12.1
15†	0	+	+	0	+11.7
16	0	+	0	0	+ 5.2
17*	+	+	+	0	+20
18*	+	+	0	0	+ 9.5
19	0	+	+	0	+16.7
20†	0	+	0	0	+ 1.1
21*	+	+	+	0	+14.8
22†	0	+	+	0	+ 9
23*	+	+	0	0	+11.3
24†	0	+	0	0	+ 3
25†	0	0	0	0	0
26*	+	0	0	0	+ 7
27†	0	0	0	0	0
28*	+	0	0	0	+ 6
29*	+	+	0	—	+13
30	0	+	0	—	+ 3.5

A, area illusion.
Vl, volume illusion.
C-L, cylinder-length illusion.
M-L, Müller-Lyer illusion.
% Il, percentage of illusion.

*In these cases an allowance of 1% has been made to eliminate the error due to the series of lines.

†An allowance of 5% has been made for the plates for a similar reason.

TABLE VIII.

Case	Obs 2 (4 trials) E	d	Obs 4 (8 tr'ls) E	d	Obs 6 (8 tr'ls) E	d	Obs 8 (8 tr'ls) E	d	Obs 10 (8 tr'ls) E	d	Obs 12 (6 tr'ls) E	d	Obs 14 (8 tr'ls) E	d	Obs 16 (2 tr'ls) E	d
1	130	2	141	8	133	4	118	4	117	4	134	3	119	5	129	5
2	133	3	130	8	136	3	125	4	115	2	137	6	134	6	129	5
3	132	3	144	3	137	6	120	5	123	6	140	2	128	5	132	3
4	133	2	128	8	133	4	122	4	118	3	132	5	135	7	122	3
5	124	0	138	5	124	1	122	3	123	3	134	5	133	5	132	3
6	118	2	127	5	119	5	115	2	121	4	119	3	123	4	117	8
7	119	0	128	6	116	3	117	2	119	2	126	4	130	3	129	0
8	115	2	127	6	123	2	117	3	118	3	122	2	127	4	117	8
9	119	0	129	6	131	2	123	2	129	4	131	5	137	3	124	0
10	118	2	130	4	113	1	117	3	120	3	126	2	128	4	117	3
11	135	2	137	3	140	4	128	3	124	5	142	6	138	8	129	10
12	128	2	137	8	130	4	119	5	118	5	133	4	121	5	124	0
13	133	2	135	9	141	5	131	3	122	3	137	3	131	6	142	3
14	137	4	136	7	134	5	128	4	117	3	132	4	129	6	124	0
15	130	4	134	5	130	5	131	2	130	2	138	4	133	8	139	0
16	120	2	127	4	118	3	118	2	122	2	116	2	127	6	122	3
17	138	4	133	7	138	7	130	3	126	4	140	5	145	3	144	0
18	130	2	137	6	131	2	123	2	113	2	127	5	118	3	129	5
19	124	0	130	7	133	2	126	2	135	3	129	3	136	3	144	0
20	120	2	128	5	115	3	122	3	119	3	120	2	121	3	119	0
21	132	3	139	6	138	3	116	4	120	4	142	4	126	6	134	0
22	124	0	135	6	129	3	117	2	126	2	137	2	132	4	139	0
23	129	0	142	6	126	4	113	3	117	4	138	3	122	6	124	0
24	119	3	132	4	117	3	114	3	121	2	133	4	128	4	124	5
25	117	3	117	6	113	1	120	1	117	2	123	4	123	2	127	3
26	129	0	125	6	125	3	118	3	115	3	131	4	119	4	124	0
27	117	5	114	6	111	2	115	2	115	4	127	5	124	3	127	3
28	124	3	120	8	122	4	115	7	112	4	134	0	117	3	122	3
29	129	5	132	5	137	2	118	3	115	3	137	8	129	2	119	0
30	117	3	121	4	113	2	110	2	118	3	124	5	123	4	112	3

mined partly to avoid disturbing influences in the serial order and partly by the convenience in manipulating the apparatus. The double fatigue order was observed. Vertical distances were compared with vertical and horizontal with horizontal.

The various cases introduced in Series III are described in Table VI. The dimension of the form measured, the form with which it was compared, and the direction of the

TABLE VIII. Continued.

Obs 18 (*2 tr'ls*)		*Obs 20* (*2 tr'ls*)		*Obs 22* (*2 tr'ls*)		*Obs 1* (*1 tr'l*)	*Ave*					
E	*d*	*E*	*d*	*E*	*d*	*E*	*E*	*d*	*Il*	*%Il*	*%D*	*Case*
122	3	134	5	132	3	134	129	4	+15	13.1	5.2	1
129	5	129	5	139	5	139	131	5	+17	15.0	4.4	2
134	0	134	5	137	8	134	133	4	+19	16.7	4.4	3
127	3	124	0	139	10	134	129	4	+15	13.1	4.4	4
122	3	124	5	139	5	144	130	3	+16	14.0	6.1	5
117	3	104	0	134	5	134	121	4	+ 7	6.1	5.2	6
122	3	124	5	124	5	139	124	3	+10	8.7	4.4	7
119	0	114	0	124	0	129	120	3	+ 6	5.2	3.5	8
119	5	124	0	129	5	139	128	3	+14	12.3	4.4	9
114	5	114	0	119	10	129	120	3	+ 6	5.2	4.4	10
127	3	139	5	144	5	144	136	5	+22	19.3	5.2	11
122	3	129	5	132	3	129	127	4	+13	11.4	4.4	12
137	8	139	5	139	10	129	135	5	+21	18.4	3.5	13
127	3	124	0	137	3	124	129	4	+15	13.1	4.4	14
129	0	129	0	139	0	139	133	3	+19	16.7	3.5	15
117	3	104	0	124	0	119	120	2	+ 6	5.2	4.4	16
137	3	139	5	149	0	139	138	4	+24	21.0	4.4	17
124	10	119	0	137	8	119	126	4	+12	10.5	5.2	18
134	0	132	3	134	0	139	133	2	+19	16.7	3.5	19
124	5	112	3	129	10	129	121	4	+ 7	6.1	3.5	20
127	8	137	3	129	0	139	132	4	+18	15.8	6.1	21
124	5	124	5	132	3	144	130	3	+16	14.0	5.2	22
124	0	137	3	132	3	129	128	3	+14	12.3	6.1	23
117	3	119	5	119	0	129	123	3	+ 9	8.0	5.2	24
114	0	117	3	119	0	129	120	2	+ 6	5.2	3.5	25
119	5	132	3	129	5	114	123	3	+ 9	8.0	4.4	26
124	5	114	0	119	0	124	119	3	+ 5	4.4	4.4	27
124	0	132	3	132	3	114	122	3	+ 8	7.0	5.2	28
134	0	124	0	132	3	129	130	3	+16	14.0	5.2	29
127	3	112	3	124	0	119	118	3	+ 4	3.5	4.4	30

Notation same as in Table I.

linear dimensions compared are given. The averages of the individual observers are found in Table VIII. This table reads across from left to right for each case and from the top down for each observer. An analysis of the illusions involved in the forms is found in Table VII, which is placed opposite Table VI for convenience in comparison. In each case the appropriate sign is given for each illusion

that enters; a plus sign signifies that the compared dimension selected should be larger than the standard dimension, and a minus sign signifies that the compared dimension selected should be smaller than the standard dimension. The same illusion may have a plus or a minus sign, depending upon the method of comparison. A plus sign in the Il. column indicates that the compared dimension was selected larger than the standard dimension.

The rather large mean variations in these tables are due to the method employed, a discussion of which will be given in connection with a later series. The very large variation given under %D is accounted for by the various degrees of knowledge and preparation represented by the observers, and by the fact that the force of the same illusion often varies greatly with different individuals.

The conclusions drawn from Series III are based upon the results obtained from twelve representative observers. They will be mentioned individually, as they represent different degrees of training and knowledge of the subject of visual illusions. Observer 2 had an extensive knowledge of the subject and was an observer of long experience; he made a strong effort not to let himself be influenced by his theories. Observers 4 to 14, even numbers inclusive, represent a class of second year students in psychology. They had practically equal knowledge of the illusions and were all about equally experienced as observers. Observer 16 was an instructor in philosophy. Observer 18 was an instructor in mathematics trained in estimating distances by the eye but he had no definite knowledge concerning any illusions except the illusion of the vertical. Observer 20, an instructor in the department of civil engineering, had never studied illusions but was a skilled draughtsman. Observer 22 was a first year student in civil engineering; he knew nothing whatever about illusions and his judgments may be regarded as more naive than the judgments of any of the other observers. Observer 1 may be classed with

observers 4 to 14 mentioned above. To none except Observer 2 was the information given that the test was upon visual illusions and, since the attention was not called to this fact, they as a rule made no conscious allowance for the illusions. With regard to the illusion of the vertical, all but Observer 20 belong to the first type of observers; but with reference to the other illusions, the Müller-Lyer illusion excepted, all but Observer 2 belong to the second type.

In the interpretation of the records, allowance must be made for an error which occurs as the result of placing the lines and plates in a series. When a line is grouped with several other lines, as in the charts described on p. 60, there is a tendency to see it shorter than it really is, regardless of direction. The amount of this underestimation has been carefully measured and will come up for discussion in the proper place under Series VII. It is necessary at this point only to note that the error in the series of lines amounts to 1%. This must be deducted from the results in all those cases in which the standard forms were compared with lines. The error in the series of plates is similar in nature but is of greater force. Its measurement is obtained from Case 25 of the present series. (See Table VIII). Here a plate is selected from the series of plates which appears equal to the height of the standard plate. Now if each form stood alone in such a comparison, there should be no illusion, but as the standard plate stands alone and the other plate is one of a series of plates, there enters an illusion of about 5%. It will be necessary to deduct this amount from all those cases in which the standard forms were compared with the height of the plates. This 5% may not be the same for all the cases but, for the present purpose, it will be necessary to assume that the error due to the series of plates is constant. These corrections have been made in Table VII by subtracting 1% for lines and 5% for plates from the original results as given in Table

TABLE IX.

Dimension of Standard Form Measured	Compared with Lines Vertical %ll	Case	Horizontal %ll	C'e	Compared with Plates Vertical %ll	C'e	Horizon'l %ll	C'e
Height of plate	7.0	26*			0	25†		
H'ght of plate, 30° down	6.0	28*			0	27†		
Cube	12.0	1*	14.0	2*	3.7	7†	5.2	8
Cube, 30° down	11.3	23*			3.0	24†		
Length of cylinder	15.7	3*	18.3	11*	9.0	5†	12.3	9
L'gth of cyl'r, 30° down	14.8	21*			9.0	22†		
Length of drawn cyl'r	17.4	13*	20.0	17*	11.7	15†	16.7	19
Diameter of cylinder	10.4	12*	12.0	4*	0	10†	6.1	6
Diam. of cyl'r, 30° down			13.0	29*			3.5	30
Diam. of drawn cylinder	9.5	18*	12.0	14*	1.1	20†	5.2	16

*In these cases an allowance of 1% was made, to eliminate the error due to the series of lines.

†An allowance of 5% was made for the plates, for a similar reason.

VIII. From the data available there is no definite means of ascertaining whether or not the width of the plate is affected when the plate is one of a series, therefore no allowance will be made in those cases where the width of the plate was compared with the standard form. In Table IX, which is a summary of Table VIII, the appropriate deduction of 1% for the lines and 5% for the plates has also been made, and it is upon this table that the discussion of this series is based.

The results of the measurements upon the standard plate, cube, cylinder, and drawn cylinder, when these forms are on a level with the observer's eyes, will be discussed first; then the corresponding measurements upon the forms when thirty degrees below the level of the eyes will be considered.

The plate will be examined first. In case 26 its height was compared with a vertical line. The result is +7%; that is, a line must be 7% higher than the plate if the two forms are to appear equal in height. In other words, if the line and plate are actually equal in height, the plate ap-

pears to the observers to be 7% higher than the line. The question—Why is this? at once arises. The effect of the illusion of the vertical is practically eliminated, for it appears in both forms and so does not change the relative sizes of the line and plate to any great extent. The forms differ from each other only in one respect; the plate has area while the line is practically without area. The difference between these forms must in some way be due to this fact. The illusion of 7% in the plate is an illusion due to area; it is on account of the width of the plate that its height appears greater than the height of the line. Hence we name this the area illusion. It is more thoroughly established in the following series and its application to the series now under discussion is therefore justified.

The cube (Fig. 1, Form 2) was introduced for the first time in Series III. In Case 1 its height was compared with a vertical line and in Case 2 its width with a horizontal line. There resulted an overestimation of 12% and 14% respectively for the two cases. Part of this is evidently due to the area illusion brought out in connection with the plate. When the cube is compared with the plate, as in Cases 7 and 8, the area illusion appears in both forms and is therefore to be disregarded. Yet in Case 7, where the height of the cube is compared with the height of the plate, there is required a plate 3.7% higher than the cube to appear equal to it; and when the width of the cube is compared with the width of the plate (Case 8), a plate 5% wider is required. The cube was so placed that only one face of it was visible to the observer; it thus had the same appearance to him as the plate, although he was fully aware that it was a cube. Now, the cube differs from the plate only in that it has the three dimensions of space; therefore any difference between the results for the two forms must be regarded as due to this fact. In other words, if the cube is judged to be larger than the plate it is because it has volume. The percentages obtained in Cases 7 and 8 and

the residuals in Cases 1 and 2 after the area illusion has been eliminated are statements of an illusion due to volume; hence we name it the volume illusion.

A line has relatively neither volume nor area. A vertical line appears equal to the height of the cube only when it is 12% taller than the cube (Case 1) and a horizontal line appears equal to the width of the cube only when it is 14% longer than the width of the cube (Case 2). These are statements of the area and volume illusions combined. The plate, which has area, appears equal in height to the height of the cube when it is 3.7% taller than the cube (Case 7) and it appears equal to the cube in width only when it is 5.2% wider than the cube (Case 8). These percentages are statements of the volume illusion in the cube. The area illusion for the height of the cube is 12% minus 3.7% or 8.3% (Case 1 minus Case 7), and for the width of the cube it is 14% minus 5.2% or 8.8% (Case 2 minus Case 8); that is, the area illusion is the difference between the results for the comparisons of the cube with lines and with plates.

Turning now to the cylinder (Fig. 1, Form 3), it is seen that when a vertical line is selected which appears equal to the length of the vertical cylinder, there is an illusion of 15.7% (Case 3); and when a horizontal line is selected which appears equal to the length of the horizontal cylinder, there is an illusion of 18.3%. Further, when the height of the plate is compared with the length of the vertical cylinder (Case 5), and the width of the plate with the length of the horizontal cylinder (Case 9), the illusions are reduced to 9% and 12.3% respectively for the two cases. That is, when the forms compared are vertical, a line must be 15.7% higher than the length of the cylinder in order to appear equal to it, whereas a plate need be but 9% higher; and when the forms compared are horizontal, a line must be 18.3% longer than the length of the cylinder in order to appear equal to it, whereas a plate need be but 12.3% wider.

This difference between the line and plate is about 6% for the vertical position and the same for the horizontal position of the forms. (Compare Case 3 with 5 and Case 11 with 9). This 6% is the area illusion in the length of the cylinder, for the motive of the area illusion found in the plate is present in the cylinder. The motive for the volume illusion found in the cube is also present in the cylinder, but in these measurements its force for the length of the cylinder is not determined directly, since it appears in combination with the illusion of cylinder length, for which no determination was made in the present series. However, in Series II this illusion was found to amount to 8% and it is probably about the same in the present series. The area illusion in the length of the cylinder is 6%; and in Case 3, where the vertical line is compared with the vertical cylinder, three illusions enter, exclusive of the illusion of the vertical, namely, the illusion of cylinder length, the area illusion and the volume illusion. The combined force of these illusions is 15.7%. Subtracting 8% for the illusion of cylinder length and 6% for the area illusion, there is a residual of 1.7% which is the volume illusion in the length of the vertical cylinder. Similarly, it is 4.3% for the length of the horizontal cylinder, when the length of the cylinder is compared with a horizontal line (Case 11). When the plate is compared with the length of the cylinder, only the illusion of cylinder length and the volume illusion enter (Cases 5 and 9). If the illusion of cylinder length is 8%, the volume illusion for the length of the vertical cylinder is 1% (Case 5), and for the length of the horizontal cylinder it is 4.3% (Case 9). Of these four statements of the volume illusion for the length of the cylinder, the two upon the length of the horizontal cylinder agree with each other and those upon the length of the vertical cylinder agree with each other, but the two determinations of the illusion for the vertical cylinder are smaller than those for the horizontal cylinder. This is due

to the fact that not all of the illusion of the vertical is excluded in the tests upon the vertical cylinder, a fact which will be fully explained in the next series; and perhaps also to unconscious correction for the illusion of the vertical.

The illusions in the diameter of the cylinder may be considered next. In Case 12 the vertical line was compared with the diameter of the horizontal cylinder and in Case 4 the horizontal line with the diameter of the vertical cylinder; the results are 10.4% and 12% respectively. This agrees very well with the tests of a similar nature upon the cube (See Cases 1 and 2), and as the same motives for illusion are present in both forms, the explanation given for one is adequate for the other. These percentages, then, are statements of the combined area and volume illusions for the diameter of the cylinder. In Case 10 the height of the plate was compared with the diameter of the horizontal cylinder. The result should give a statement of the volume illusion, but as recorded in Table IX it is zero. It will be remembered that this is one of the cases in which an allowance was made for the series of plates, and in this instance it would seem that this 5% was too great an allowance. Again, throughout Table IX there is brought out a general tendency for the illusion to be somewhat stronger when the dimensions compared are horizontal than when they are vertical. The reason for this will be given in the next series. It may be, then, for these two reasons, namely—too great an allowance for the error in the series of plates and the smaller illusion when the compared forms are vertical, that the volume illusion for the diameter of the cylinder is not brought out in Case 10. That it is present is indicated in two ways: first, by the fact that the 10.4% of Case 12 is too great for the area illusion alone, consequently something of the volume illusion must be involved; and second, when the width of the plate is compared with the diameter of the

vertical cylinder (Case 6) the volume illusion for the diameter of the cylinder is 6%. If the volume illusion is 6% for the diameter of the cylinder, then the area illusion for the diameter of the cylinder in Cases 12 and 4, which represent the combined effect of the two illusions, is 4.4% and 6% respectively for the two cases.

In connection with Series II it was found that the diameter of the cylinder was overestimated by 8%, and this illusion was termed the illusion of cylinder diameter. It is evident that the volume illusion of the present series and the illusion of cylinder diameter of Series II are precisely the same illusion; in other words, the overestimation of cylinder diameter is due to the volume illusion.

If the measurements upon the length of the drawn cylinder are compared with the corresponding measurements upon the length of the metal cylinder, it will be observed that the same general laws hold for the one as for the other. (Compare Cases 13 and 3, 17 and 11, 15 and 5, 19 and 9). The diameter of the drawn cylinder is also judged to be practically the same as the diameter of the metal cylinder. (Compare Cases 18 and 12, 14 and 4, 20 and 10, 16 and 6). These facts show that the same illusions are present in both the real and the drawn cylinders. In other words, the illusions persist as long as the form is perceived as a cylinder, although in reality it may not be a cylinder. The significance of this will be brought out in the next series.

The tests considered in the foregoing discussion were made with the forms on a level with the eyes of the observers. Corresponding measurements were made upon some of the forms when these were placed thirty degrees below the line of vision. For the height of the plate the angle seems to make no difference in the results. (Compare Cases 26 and 28, and 25 and 27). The same may be said of the cube (Cases 1 and 23, and 7 and 24), and also for the length of the cylinder (Cases 3 and 21, and 5 and 22). When measurements are made upon the diameter of the

cylinder in this position, a Müller-Lyer effect should appear causing an underestimation of the diameter. For some reason this is not brought out in Case 29, but it appears in Case 30. (Compare Cases 4 and 29, and 6 and 30). On the whole the measurements upon the forms in this second position support those made upon the forms in the first position.

The illusions involved in the standard forms in this series are shown in the analysis of them given in Table VII. The following general conclusions may be drawn:

The height of the plate is overestimated; this is the area illusion.

The area illusion also appears in the height and width of the cube.

It also appears in the length and diameter of the cylinder and in the length and diameter of the drawn cylinder.

In addition to the area illusion, the height and width of the cube are overestimated. This is the volume illusion.

The volume illusion appears in the length and diameter of the cylinder and it is also present in the length and diameter of the drawn cylinder.

The Müller-Lyer illusion sometimes affects the perception of the diameter of the cylinder.

Series IV

Series IV followed closely upon Series III, the records being made in January, 1901. The aim of the series was three-fold: first, to obtain measurements upon the illusion of the vertical for some typical geometrical forms; second, to test an hypothesis which would explain the volume illusion; and third, to study the illusions in some new forms. In addition to the plate, cube, cylinder, and drawn cylinder of Series III, the standard forms consisted of a sphere, a disk, a circle, a drawn square, and an ellipse. (See Fig. 1, Forms 4, 8, 12, 13, and 14, respectively). Each

form was compared with lines; the width or horizontal dimension of each was compared with a horizontal line, and the height or vertical dimension with a vertical line and a horizontal line.

The same method as for the lines in the previous series was employed. Six different series of lines were used with which to compare the standards, thus providing against the observer's selecting from memory. The backgrounds upon which the lines were drawn were also frequently turned so that the position of the top and bottom was reversed; and a different series of lines was used for each determination. The relative position of the standard and compared forms was the same as in the previous series. Two determinations were made for each of thirty-two cases with each of twenty observers.

The list of cases introduced into the series and the positions of the standard and compared forms is found in Table X.

In Table XI the signs for the several illusions which influence the records are given, and also the percent of illusion. In this table allowance was made for the 1% error due to the series of lines as explained in the previous series. The interrogation point is placed where there is some doubt as to the presence of the illusion. The difference in the signs for the illusion of the vertical will be explained later. The discussion of the series is based chiefly on Table XI.

In Table XII the individual estimates of the twenty observers are given. There were two trials for each case with each observer and the average of these forty trials is given, together with the amount of the illusion in millimeters and in percentages; also the %D. The individual mean variations are not given, since they can easily be made out from the table. In this table no deduction is made for the 1% error due to the series of lines.

The twenty observers in this series were selected from

TABLE X.

Case	Dimension of Standard Form Measured	D. St. F.	D. Comp. Line	No. in Fig. 1
1	Height of plate	Vertical	Vertical	1
2	Width of plate	Horizontal	Horizontal	1
3	Height of plate	Vertical	Horizontal	1
4	Height of cube	Vertical	Vertical	2
5	Width of cube	Horizontal	Horizontal	2
6	Height of cube	Vertical	Horizontal	2
7	Height of set-in cube	Vertical	Horizontal	2
8	Width of set-in cube	Horizontal	Horizontal	2
9	Height of set-in cube	Vertical	Vertical	2
10	Diameter of sphere	Horizontal	Horizontal	4
11	Diameter of sphere	Vertical	Horizontal	4
12	Diameter of sphere	Vertical	Vertical	4
13	Diameter of disk	Vertical	Vertical	8
14	Diameter of disk	Horizontal	Horizontal	8
15	Diameter of disk	Vertical	Horizontal	8
16	Long axis of ellipse	Vertical	Vertical	14
17	Long axis of ellipse	Vertical	Horizontal	14
18	Long axis of ellipse	Horizontal	Horizontal	14
19	Height of drawn square	Vertical	Vertical	13
20	Width of drawn square	Horizontal	Horizontal	13
21	Height of drawn square	Vertical	Horizontal	13
22	Diameter of circle	Vertical	Vertical	12
23	Diameter of circle	Horizontal	Horizontal	12
24	Diameter of circle	Vertical	Horizontal	12
25	Length of cylinder	Horizontal	Horizontal	3
26	Diameter of cylinder	Vertical	Horizontal	3
27	Length of cylinder	Vertical	Horizontal	3
28	Diameter of cylinder	Horizontal	Horizontal	3
29	Length of drawn cylinder	Horizontal	Horizontal	15
30	Diameter of drawn cylinder	Vertical	Horizontal	15
31	Length of drawn cylinder	Vertical	Horizontal	15
32	Diameter of drawn cylinder	Horizontal	Horizontal	15

D. St. F., the direction of the dimension measured in the standard form.

D. Comp. Line, the direction of the line with which the standard was compared.

The number of the form in Fig. 1 is given in the last column.

TABLE XI.

Case	*A*	*Vl*	*C-L*	*M-L*	*V*	*%Il*
1	+	0	0	0	—	+ 2.5 (+3.5)
2	+	0	0	0	0	+ 7.7
3	+	0	0	0	+	+13.0
4	+	+	0	0	—	+ 5.0 (+7.4)
5	+	+	0	0	0	+10.4
6	+	+	0	0	+	+14.0
7	+	+	0	0	+	+14.0
8	+	+	0	0	0	+ 9.5
9	+	+	0	0	—	+ 6.0 (+7.5)
10	+	+	0	—?	0	+ 9.5
11	+	+	0	—?	+	+10.4
12	+	+	0	—?	—	+ 2.5 (+7.5)
13	+	0	0	—?	—	— 2.0 (+3.0)
14	+	0	0	—?	0	+ 6.0
15	+	0	0	—?	+	+ 7.0
16	+	0	0	—	—	— 3.6 (—0.2)
17	+	0	0	—	+	+ 6.0
18	+	0	0	—	0	+ 3.4
19	+	0	0	0	—	+ 3.4 (+6.9)
20	+	0	0	0	0	+ 9.5
21	+	0	0	0	+	+12.0
22	+	0	0	—?	—	— 1.0 (+4.0)
23	+	0	0	—?	0	+ 6.0
24	+	0	0	—?	+	+ 7.0
25	+	+	+	0	0	+16.5
26	+	+	0	0	+	+15.0
27	+	+	+	0	+	+20.0
28	+	+	0	0	0	+12.0
29	+	+	+	0	0	+17.4
30	+	+	0	0	+	+13.0
31	+	+	+	0	+	+18.3
32	+	+	0	0	0	+11.3

A, area illusion.
Vl, volume illusion.
C-L, cylinder-length illusion.
M-L, Müller-Lyer illusion.
V, illusion of the vertical.
%Il, percent of illusion.
1% for each case was deducted from the results as given in Table XII to eliminate the error due to the series of lines.

TABLE XII.

	Case	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*
Obs	1	119	129	129	119	134	129	134	134	139	129	124	114	109	114	119	114
		119	129	129	124	139	129	134	134	124	119	119	114	109	114	114	109
	3	124	129	124	109	119	119	124	114	124	119	129	119	109	119	119	104
		134	134	139	124	134	144	134	124	124	139	134	124	139	139	144	129
	5	114	109	129	114	124	109	114	119	109	104	109	104	104	104	109	94
		104	104	104	104	109	104	104	109	104	99	104	94	94	99	99	94
	2	104	119	119	114	134	129	124	114	109	119	114	109	104	109	114	104
		104	114	114	104	114	119	114	114	104	114	119	104	104	114	114	104
	7	149	129	134	124	134	144	144	134	129	124	134	129	114	124	134	124
		124	129	144	134	134	144	139	134	139	129	134	124	119	124	124	119
	4	134	129	139	129	134	144	139	134	129	124	134	129	114	129	129	104
		124	129	134	124	134	144	144	129	134	129	129	124	119	119	134	109
	6	124	129	134	134	119	134	139	134	129	129	134	129	129	129	134	104
		119	114	134	124	134	139	124	129	124	129	124	129	114	119	129	109
	9	109	114	149	129	124	149	144	124	114	139	144	124	109	134	124	109
		109	129	139	114	139	139	139	134	119	134	144	114	109	129	134	109
	8	119	119	129	114	124	124	129	124	119	114	114	114	114	124	124	124
		119	129	134	129	129	129	124	134	134	129	129	124	119	124	129	119
	10	114	129	134	119	129	124	129	124	114	129	124	109	109	124	124	109
		114	129	134	119	134	139	139	129	129	129	129	114	114	134	129	114
	12	124	139	139	139	129	134	134	134	124	134	139	129	124	144	124	129
		129	144	139	139	139	144	139	144	124	144	139	129	134	139	134	129
	14	124	139	129	139	129	139	144	144	139	149	149	149	129	134	144	124
		134	144	149	139	144	144	144	144	139	149	149	149	139	149	144	134
	11	109	119	119	119	124	129	129	119	114	129	129	119	109	124	119	109
		129	129	144	129	134	134	139	129	129	124	129	124	119	129	119	114
	16	129	114	134	119	119	134	124	119	114	129	129	114	114	129	124	109
		119	129	134	119	119	129	134	124	119	129	129	124	114	124	129	109
	18	114	119	119	119	119	114	124	124	114	119	129	114	109	119	124	114
		114	124	124	114	129	119	119	124	114	124	114	114	114	109	126	104
	13	119	119	119	114	114	119	129	119	114	129	124	119	104	114	109	104
		114	114	119	119	119	124	129	124	124	124	119	114	114	114	114	104
	20	109	134	129	114	124	129	134	134	124	134	134	109	114	134	129	114
		119	129	129	124	129	129	134	124	119	129	134	114	114	134	124	114
	15	104	114	124	104	109	124	129	114	114	109	114	94	94	114	114	109
		104	114	124	114	114	124	119	114	114	109	114	94	104	109	114	94
	22	114	114	114	119	129	139	129	119	119	114	119	109	109	99	114	104
		114	114	124	114	119	124	119	119	114	114	114	109	104	109	104	114
	24	109	114	119	119	129	129	134	129	119	124	119	119	109	109	109	104
		114	129	139	119	129	129	129	129	119	129	129	119	104	119	124	109
Ave		118	124	130	121	127	131	131	126	122	126	127	118	113	122	123	111
I1		4	10	16	7	13	17	17	12	8	12	13	4	—1	8	9	—3
%I1		3.5	8.7	14	6	11.4	15	15	10.5	7	10.5	11.4	3.5	—0.9	7	8	—2.6
%D		7	7	7	6	6	7	6	6	7	7	8	8	7	9	7	8

Where no sign is given in the I1 and the %I1 columns the plus sign is understood. Other notation same as in Table I.

TABLE XII. Continued.

17	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*	*27*	*28*	*29*	*30*	*31*	*32*	*Obs*
109	109	114	119	129	109	114	119	134	139	139	129	139	129	134	114	1
114	114	119	119	119	109	129	114	129	134	134	134	134	119	129	124	
124	129	124	119	114	124	124	139	139	144	149	144	144	139	139	134	3
129	129	129	139	134	124	134	129	139	149	139	139	134	134	129	129	
99	99	109	114	114	94	99	104	114	114	119	99	129	104	119	114	5
104	104	109	104	114	94	119	104	124	104	124	109	124	99	114	99	
114	109	109	119	109	109	119	119	119	119	134	119	129	119	134	119	2
124	119	109	114	119	104	114	114	124	119	129	124	129	119	129	119	
129	124	119	124	134	104	124	124	134	149	149	149	134	129	149	129	7
129	124	134	134	139	119	119	119	149	144	149	139	134	144	144	129	
119	114	119	134	139	114	129	129	139	134	144	129	144	139	149	129	4
119	114	129	134	139	124	129	129	144	134	144	129	144	139	149	139	
119	119	139	129	139	119	129	129	139	139	144	134	134	139	149	129	6
129	114	104	129	144	119	129	129	134	139	144	134	144	129	144	129	
119	119	109	139	144	114	129	139	139	144	149	134	144	134	144	134	9
129	129	119	129	139	109	129	129	144	134	149	134	134	129	144	129	
124	119	119	124	129	119	129	124	139	129	129	124	139	134	129	124	8
129	129	119	134	129	114	124	124	129	134	134	129	129	129	134	134	
124	124	114	124	129	114	124	119	119	124	129	129	134	129	134	129	10
124	129	124	129	134	114	129	129	134	134	144	129	134	134	129	129	
134	134	124	139	134	119	129	129	139	119	144	129	144	129	139	124	12
139	139	129	139	129	129	134	134	134	139	144	134	139	119	134	134	
124	134	134	144	149	129	134	144	144	149	144	139	144	139	144	144	14
149	139	139	144	149	134	139	144	149	149	149	149	149	144	149	139	
124	114	129	129	129	114	129	124	139	144	139	144	139	144	144	144	11
129	129	119	129	129	124	134	134	149	139	144	139	134	144	144	144	
114	114	119	114	129	114	114	114	134	129	144	124	129	129	129	129	16
129	114	129	129	134	114	114	119	134	129	134	129	134	129	139	124	
124	109	109	114	124	104	109	114	129	134	129	134	129	119	119	129	18
109	119	109	129	124	109	124	119	129	124	129	129	129	124	124	124	
109	109	114	109	114	114	119	114	119	114	124	119	144	129	124	124	13
114	114	114	109	114	114	114	129	129	124	129	119	124	119	129	129	
129	124	119	134	134	114	119	124	134	139	144	134	139	139	134	144	20
129	119	119	139	134	114	129	129	144	139	139	139	144	149	144	134	
109	114	104	119	124	104	114	119	129	124	134	119	119	114	134	114	15
114	109	104	114	119	109	114	114	129	124	129	114	124	119	134	114	
114	119	114	119	124	114	104	114	129	114	139	124	134	124	134	124	22
129	124	114	114	124	114	109	109	129	124	139	109	129	129	144	134	
114	114	119	119	114	104	104	104	124	119	129	114	134	139	134	124	24
119	114	114	124	134	104	114	114	134	129	139	124	139	134	129	139	
122	119	119	126	129	114	122	123	134	132	138	129	135	130	136	128	
8	5	5	12	15	0	8	9	20	18	24	15	21	16	22	14	
7	4.4	4.4	10.5	13	0	7	8	17.5	16	21	13	18.4	14	19.3	12.3	
7	6	6	5	7	6	7	7	6	8	6	7	7	7	7	6	

TABLE XIII.

Dimensions of Standard Forms Measured in Terms of Lines	*Direction*	*Series III 12 Observers* Case	%Il	*Series IV 20 Observers* Case	%Il
Height of plate	Vertical	26	+ 7.0	1	+ 2.5
Height of cube	Vertical	1	+12.0	4	+ 5.0
Width of cube	Horizontal	2	+14.0	5	+10.4
Length of horizontal cyl'r	Horizontal	11	+18.3	25	+16.5
Diameter of vertical cyl'r	Horizontal	4	+12.0	28	+12.0
L'gth of horiz'l drawn cyl'r	Horizontal	17	+20.0	29	+17.4
Diam. of vert'l drawn cyl'r	Horizontal	14	+12.0	32	+11.3

the class in elementary psychology. The degree of knowledge of the illusion possessed by each observer was so nearly the same that a discussion of the observers individually will be unnecessary. The subject of visual illusions had been studied in class but a short time before and the results bring out a tendency to allow for the illusion of the vertical in those cases in which the two compared forms were both vertical. The illusion of the vertical was the only illusion studied in class which would apply to the present series. The reaction of the observers against this illusion and its significance will be considered later.

Seven of the cases were repeated from Series III. The results of these cases are given in Table XIII to facilitate a comparative study. The two series were made at different times and with a quite different set of observers but the methods of procedure were the same. It is readily noticed that, exclusive of the measurements upon the height of the cube and plate, there is a very satisfactory agreement in the results for the two series. The explanation of the two exceptions just noted will be given subsequently. Had there been a considerable disparity in the results, this could have been explained by the fact that the force of the same illusion often varies greatly with different individuals. Since, however, the results are in close agreement, this may be taken as an indication of the reliability of the tests

and the relative constancy of the illusions involved. The existence of the illusions as they are indicated for the forms in Table XIII, may be regarded as established beyond reasonable doubt. These forms have been systematically discussed under Series III, and a repetition of that discussion is therefore unnecessary here.

One of the main purposes of Series IV was the study of the illusion of the vertical. To obtain measurements of this illusion in the various forms, the same dimension, alternately in the vertical and in the horizontal position, was compared with a horizontal line; the difference between the two results gave the desired measurements. The illusion as determined for the different forms is as follows:

The plate	5.2%	Case 3 minus Case 2
The set-in cube	4.5%	Case 7 minus Case 8
The cube	3.6%	Case 6 minus Case 5
Length of the vertical cylinder	3.5%	Case 27 minus Case 25
Diameter of the horizontal cylinder	3.0%	Case 26 minus Case 28
Diameter of the horizontal drawn cylinder	2.7%	Case 30 minus Case 32
Long axis of the ellipse	2.6%	Case 17 minus Case 18
The drawn square	2.5%	Case 21 minus Case 20
Diameter of the disk	1.0%	Case 15 minus Case 14
Diameter of the circle	1.0%	Case 24 minus Case 23
Diameter of the sphere	0.9%	Case 11 minus Case 10
Length of the vertical drawn cylinder	0.9%	Case 31 minus Case 29

The results indicate that the illusion of the vertical varies with the form; in general, excluding the circle and related forms in which it is checked, it is stronger for the less complex forms. The line was not studied in this series but in the other series the illusion of the vertical is on the whole about 1% stronger in the line than in the plate. The line with an illusion of 6% would therefore head this list of forms which are arranged in the order of the strength of the illusion. These figures are low because the observers had just heard the lectures upon geometrical illusions, as was stated, and the subject was fresh in their minds. The

average is also lowered by the exceptional reaction of a few against the illusion.

A most conclusive, systematic corroboration of this law of the variation of the illusion of the vertical with the form is found in the fact that throughout Series IV the results are smaller when both the standard and compared forms are vertical than when they are both horizontal. This is seen by comparing Cases 1, 4, 9, 12, 13, 16, 19, and 22, in which the forms were vertical, with Cases 2, 5, 8, 10, 14, 18, 20, and 23, in which the forms were horizontal. In the investigation up to this point, whenever the illusion of the vertical entered into the standard and the compared forms, its effect was regarded as eliminated from the results. This, however, appears in view of the present series, not to have been justifiable. Now, it is readily seen that when the vertical line is compared with the height of some standard form, for which the force of the illusion of the vertical is less than for the line, not all of the illusion of the vertical is eliminated from the result; only so much is eliminated as occurs in the standard form and there is still present the amount by which the illusion in the line is greater than in the standard form. For instance, the illusion of the vertical is 6% for the line, and, according to the present series, it is 1% for the circle. There is a difference of 5% in its force for the two forms. The consequence of this difference is that a line 5% shorter is selected as equal to the height of the circle, whereas, if the illusion were of the same strength for the two forms, so far as the illusion of the vertical alone is concerned, a line of the same height as the vertical diameter of the circle would be selected. The amount by which the illusion of the vertical for a given form is less than the same illusion for the line must be added to the result of the comparison of the vertical line with the height of that form. In order to eliminate the effect of the illusion of the vertical when two vertical dimensions are compared, it is neces-

sary to introduce corrections for the variation with form. More specifically, in Case 4, in which the height of the cube was compared with a vertical line, 2.4% must be added to the result, this 2.4% being the difference between the illusion of the vertical for the cube, (3.6%), and for the line, (6%). Case 4 then reads 7.4%. In like manner, 1% must be added to Case 1, 1.5% to Case 9, 5% to Case 12, 5% to Case 13, 3.4% to Case 16, 3.5% to Case 19, and 5% to Case 22. After these additions have been made the cases read as indicated in the parentheses in Table XI.

The records of Series III also indicated a difference in the force of the illusion of the vertical for the standard and compared forms and attention was called at the time to the fact that not all the illusion of the vertical was eliminated from the result. (Table IX. Cf. Cases 13 and 17, 3 and 11, etc.).

The variation with form introduces very grave complications in measurement. There is need of extensive experiments to determine the general laws for this variation more comprehensively and in detail in order that it may be taken into consideration in all measurements in which it is involved.

If this difference in the force of the illusion of the vertical for the standard and compared forms were the only disturbing factor, then, due allowance having been made for it, the measurements taken when these forms were both in the vertical position should agree with those taken when both the standard and compared forms were in the horizontal position. That is, Case 1 should agree with Case 2, Case 4 with Case 5, Case 9 with Case 8, Case 12 with Case 10, Case 13 with Case 14, Case 16 with Case 18, Case 19 with Case 20, and Case 22 with Case 23. The differences in the results for these eight pairs of cases are respectively, 4.2%, 3%, 2%, 2%, 3%, 3.6%, 2.6%, and 2%. An adequate explanation for these residuals may be found in the hypothesis that in all those cases in which two vertical distances were

compared, the observers made an allowance for the illusion of the vertical by selecting a shorter line, and this allowance may have been in either the standard or the compared form.

There has frequently been occasion to note, in the course of this research, that an observer often makes an allowance for, or reacts against, a known illusion without being in the least conscious of such a reaction or allowance. Careful questioning of each observer in this series met with the response that no allowance had been made consciously. But that such allowance did play a part is most clearly brought out in the cases noted. It is important to guard against this unconscious allowance for an illusion, although there is often no means of determining whether or not it is present. It is well to ascertain, if possible, the exact amount of preparation or foreknowledge of the observer; the more naive he is the better. The fact that introspection reveals no trace of this unconscious allowance points to the need of experimenting by objective methods upon observers in a naive state of mind.

A characteristic feature of this unconscious reaction against an illusion is the fact that there is often exhibited in it a lack of logical consistency. For instance, throughout Series IV the observers apparently made allowance only in those cases in which two vertical distances were compared. (Cases 1, 4, 9, 12, 13, 16, 19, and 22). The allowance, if made at all, should have been made in those cases in which the standard form was vertical and the compared form horizontal. This would tend to show that the allowance was not consciously made. The principle that vertical distances are overestimated was familiar to the observers and this principle was more strongly suggested in those cases where the allowance was made than in any of the other cases. This inconsistency was due partly to the observers' lack of training in the experimental method. This unconscious allowance was not made by every individual observer in Series IV, but the averages of the estimates of the ob-

servers as a class indicate plainly the presence of the general tendency.

In connection with Series III, a possible explanation of the volume illusion was suggested by the comparison of the judgments upon the plate and the cube. The forms were placed before similar backgrounds, and as only one face of the cube was visible and this of exactly the same size as the plate, the objective conditions in the perception of the two forms were almost identical. The side of the cube was judged to be larger than the plate and this was explained as being due to the volume of the cube. In every normal perception there is a subjective and an objective element. Since the objective conditions in the two perceptions mentioned were so similar, it is plain that the volume illusion must be explained from the subjective side of the perception. Accordingly a crucial test of it was introduced, based upon the following hypothesis, stated in terms of representative forms: The face of a cube looks larger than a square plate of the same size on account of the association of volume with the former. That is, experience has taught the observers that voluminal objects are larger than those without volume and his judgments are influenced by this knowledge. From this point of view, then, the volume illusion is not due to physiological causes, but is of entirely subjective origin and to be classed as an illusion of association.

The experiment for the testing of this theory was arranged according to the following plan: It is plain that if objectively the plate and cube were indistinguishable and the observer had no means of knowing that two separate forms were being studied, the results of a sufficient number of trials upon each of the two forms, other things being equal, would be the same. If, on the other hand, the same tests were made with the forms still objectively indistinguishable but with this subjective difference that the observer knew when he was looking at the plate and at the cube, any difference between the results for these two forms would be

due to this fact. The nature of this difference in the subjective element in the perception of the two forms is indicated by a study of the forms themselves; the plate has area alone, the cube has area and volume. The two forms differ in no other respects. The area of the face of each is directly perceived by the sense of sight; the volume of the cube is not, but the observer knows that he is looking at a cube. The idea of volume is present in the form of the judgment "there is more to the cube than to the plate" and the effect of this is brought out in the experiments.

The height of the plate was compared with a vertical line in Case 1 and with a horizontal line in Case 3, while in Case 2 the width of the plate was compared with a horizontal line. Cases 9, 7, and 8 respectively represent parallel measurements upon the cube. The cube was closely fitted into an opening in a background, like the one upon which the plate was fastened, so that only the front face was visible and this was flush with the plane of the background. Placed in this way the plate and the cube could not be distinguished by the observer. But when each form was presented to him he was told whether he was looking at the plate or at the cube. In fact he saw the cube adjusted in place each time. In this way the conditions necessary to test the theory that the volume illusion is due to the idea of volume were met.

When in Case 1 the height of the plate was compared with a vertical line, the result was 2.5%. The result of the same measurement upon the set-in cube was 6%. There is a difference of 3.5% between the two cases. When the height of the plate is compared with a horizontal line, Case 3, the result is 13%. The same test upon the height of the set-in cube, Case 7, gave 14% as a result, or a difference of 1%. The result of the comparison of the width of the plate with a horizontal line (Case 2) is 7.7%, and the same for the set-in cube (Case 8) is 9.5% or a difference of 1.8%. These differences of 3.5%, 1%, and 1.8% represent the effect of the idea of volume. The objective conditions in the three

pairs of cases were the same. The subjective conditions differed in that in connection with the cube there was the idea of volume, but with the plate there was no such idea present and herein lies the reason that the cube was judged to be larger than the plate. The hypothesis above stated, then, stands a crucial test. The illusion of volume is an association illusion, due to the presence of the idea of volume. It does not necessarily follow from this that the illusion is due to any conscious judgment upon the part of the observer. On the contrary it is quite probable that in his experience the tendency to interpret voluminal objects as larger than those without volume has become an habitual and well fixed unconscious process.

The 3.5%, 1%, and 1.8% given above are, however, not expressions of the full force of the volume illusion. One of the chief conditions of the presence of the illusion is that the motives be unrestricted. The illusion is stronger when the image of volume in the form is clear than when it is vague.[1]

In both Series III and IV, it has been shown that the results upon the drawn cylinder are practically the same as those upon the metal cylinder. This fact is another strong proof that the volume illusion is due to the idea of volume. The drawn cylinder is perceived as a solid; that is, it is thought of as having volume. The idea of volume is present and accordingly the same illusions appear as in the regular cylinder; the volume illusion appears even though the volume of the form is only suggested.

So much for the volume illusion.[2] The area illusion has

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[1] The conditions of this test are parallel to those upon the weight illusion in which it was demonstrated that the illusion is stronger when the size of the object that is lifted is seen than when it is merely remembered. SEASHORE, *Measurements of Illusions and Hallucinations in Normal Life*. Studies from the Yale Psychol. Lab. 1895, III, p. 11.

[2] The presence of the volume illusion in the wires used in Series II may explain the discrepancy noted for those cases (Vid. p. 59).

been shown to be connected in some way with the presence of area in the form. The analogy between the conditions for the area illusion and the conditions for the volume illusion is complete; therefore the area illusion is an association illusion and of the same general nature as the volume illusion.

Turning now to the new forms, the sphere will be considered first. In Case 10 the horizontal diameter of the sphere was compared with a horizontal line, and in Case 12 the vertical diameter with a vertical line. The results are +9.5% and +2.5% respectively. These results should be compared with the corresponding measurements upon the width (Case 5), and height (Case 4), of the cube, the results of which are +10.4% and +5% respectively. It is seen that the illusions in the sphere are almost as strong as those in the cube. In this latter form the overestimation of the height and width was explained by the area and volume illusions. The sphere and cube are both typical geometrical solids and the nature of the area and volume illusions is such that they are applicable to all such objects. The overestimation of the size of the sphere is caused, then, by the area and volume illusions. However, in the sphere there may be involved something of the Müller-Lyer illusion with a tendency to bring about a slight underestimation of the diameter. It is possible that for this reason the measurements upon the sphere are somewhat smaller than those upon the cube. The sphere also has smaller area and volume than the cube and this may have reduced the force of the illusions slightly. The 9.5% of Case 10 is a statement of the combined area and volume illusions for the sphere, or better, it is the amount by which these illusions outweigh the Müller-Lyer effect. In Case 12 the result obtained is not representative of the full force of the illusions, for it is one of those cases in which the observers made allowance, and also one in which the illusion of the vertical was less for the standard than for the compared form.

The disk bears the same relation to the plate as the sphere to the cube. The area illusion would tend to cause an overestimation of the diameter of the disk. If the Müller-Lyer illusion enters, it has an opposite effect. As in the case of the sphere, in the measurements upon the vertical diameter, correction must be made for the allowance of the observers and also for the variation of the illusion of the vertical with the form. In Case 14, the result of which is +6%, there is obtained a statement of the amount by which the area illusion in the disk outweighs the Müller-Lyer illusion. The effect of the latter illusion cannot be determined from these records.

A measurement of the illusions in the ellipse is obtained from Case 18, where the horizontal line was compared with the long axis in a horizontal position. The result is +3.4%, which represents the amount by which the area illusion is stronger than the Müller-Lyer illusion.

The drawn square is but a variation of the plate and the same illusions are involved in it as in the plate. It was inserted into this series partly as a control upon the plate and partly to determine whether the mode of definition of the area had any effect upon the resulting illusions. The black area of the plate, being throughout in contrast to the background, was more clearly defined than the area of the drawn square which was merely limited by lines. By comparing Case 20 with Case 2, and Case 19 with Case 1, it is seen that the drawn square is judged to be about 1% larger than the plate, both in height and width. A possible explanation of this is that the apparent size of the plate is reduced by irradiation. The principle of irradiation has not been taken into account before as its effect was practically eliminated. There may also be physiological reasons for the difference, since the eye movements are quite different in the perception of the two forms.

The circle and the disk stand in the same relation to each other as the plate and drawn square. In Case 23 the

horizontal diameter of the circle was compared with a horizontal line; the result is the same as for the disk, +6%. For some reason the effect of irradiation is not brought out in the results.

The results of Series IV seem to warrant the following statements:

The main conclusions of Series III are corroborated by fresh evidence. (Table XIII).

The illusion of the vertical varies with the form, decreasing with the increase in the complexity of the form. (See order of forms and exceptions to this rule, p. 81).

Knowledge of an illusion causes the observer to introduce a correction for it unconsciously; such unconscious systematic allowance for the illusion of the vertical is demonstrated.

The volume illusion is shown to be an association illusion due to the idea of volume; the volume may be either real, or suggested, as in a drawing. The volume illusion varies with the prominence of the idea of volume.

By analogy, the area illusion is an assocation illusion due to the idea of area.

In the new forms, the area illusion and the volume illusion are demonstrated for the sphere, and the area illusion for the disk, ellipse, drawn square, and circle.

Series V

The tests in this series were made in the summer of 1901. The purpose was to obtain, by different methods, further measurements upon the illusion of the vertical for the line, and also to determine the area and volume illusions in some new forms. The method employed in all but the first four cases was the same as that used in Series IV. The standard forms consisted of a line, a cone, a pyramid, a triangle, a cylinder, a cube, a plate, a cone and cylinder, a pyramid and cube, and a triangle and plate.

(Fig. 1, Forms 16, 7, 6, 5, 3, 2, 1, 11, 10, and 9 respectively).

The observers were five members of the class in experimental psychology during the summer session of the University, one graduate student (Obs. 6), and an assistant-professor (Obs. 2). With reference to the illusion of the vertical these observers were of the first type, but with reference to the area and volume illusions they were of the second type.

Seventeen cases were introduced into the series and eight determinations upon each case were made for each observer in the double fatigue order. The different cases are stated in Table XIV; in Table XV the signs of the illusions involved and the results are given. As before, 1% has been subtracted to eliminate the error due to the series of lines in Cases 5 to 11 and 15 to 17 inclusive. The averages of the eight trials for each observer are given in Table XVI.

One purpose of this series was to measure the effect of the methods employed upon the illusion of the vertical, and the first five cases were arranged to test this. The methods and results for these cases are as follows:

Case 1. Method of production: to indicate on a horizontal line a distance equal to the standard vertical line. Two lines one millimeter wide were drawn at right angles upon a large sheet of light colored cardboard. The vertical line was 114 mm. in length and the horizontal line much longer. The problem was to cover with a strip of cardboard as much of the extra length of this line as was necessary to make the portion in sight appear equal to the standard vertical line. The only illusion known to enter is the illusion of the vertical, with a tendency to produce an overestimation. The result is +3.5%.

Case 2. Method of production: to indicate on a vertical line a distance equal to the standard horizontal line. This is the reciprocal of Case 1, the standard line now being

TABLE XIV.

Case	
1	To lay off distance on horizontal line equal to vertical line.
2	To lay off distance on vertical line equal to horizontal line.
3	To select horizontal line equal to vertical line.
4	To select vertical line equal to horizontal line.
	To select a horizontal line equal to:—
5	Length of line, vertical.
6	Altitude of cone, vertical.
7	Diameter of base of cone, horizontal.
8	Altitude of pyramid, vertical.
9	Base of pyramid, horizontal.
10	Altitude of triangle, vertical.
11	Base of triangle, horizontal.
12	Length of cone and cylinder, vertical.
13	Length of pyramid and cube, vertical.
14	Length of triangle and plate, vertical.
15	Length of cylinder, vertical.
16	Height of cube, vertical.
17	Height of plate, vertical.

horizontal; the method was otherwise the same. The effect of the illusion of the vertical is to producé an underestimation. The vertical line is made 2.6% too short. In these first two cases the observers probably introduced some correction for the illusion of the vertical, especially in Case 2, where by varying the vertical dimension more attention is called to the illusion.

Case 3. Method of selection: to select a horizontal line equal to the standard vertical line. Two lines were drawn at right angles upon each of twelve cards of a light tint and about 28 cm. square. One of the lines on each card was the standard, 114 mm. in length; the other line varied on the different cards by five-millimeter steps from 94 mm. to 149 mm. The observer was told to select the card upon which the two lines appeared equal, according to the method used in Series I. In this case the horizontal line selected should be too long; it was selected too long by 4.4%.

Case 4. Method of selection: to select a vertical line

TABLE XV.

Case	*A*	*Vl*	*C-L*	*M-L*	*V*	*L*	*%Il*
1	0	0	0	0	+	0	+ 3.5
2	0	0	0	0	—	0	— 2.6
3	0	0	0	0	+	0	+ 4.4
4	0	0	0	0	—	0	— 4.4
5	0	0	0	0	+	0	+11.0*
6	+	+	0	—	+	0	+16.0*
7	+	+	0	—?	0	0	+ 7.7*
8	+	+	0	—	+	0	+13.0*
9	+	+	0	—?	0	0	+ 8.6*
10	+	0	0	—	+	0	+11.0*
11	+	0	0	—?	0	0	+ 7.0*
12	+	+	+	—	+	—	+11.4
13	+	+	0	—	+	—	+12.0
14	+	0	0	—	+	—	+10.5
15	+	+	+	0	+	0	+20.0*
16	+	+	0	0	+	0	+16.5*
17	+	0	0	0	+	0	+15.0*

A, area illusion.
Vl, volume illusion.
C-L, illusion of cylinder length.
M-L, Müller-Lyer illusion.
V, illusion of the vertical.
L, illusion of length.
%Il, percentage of illusion.
*In these cases an allowance of 1% has been made to eliminate the error due to the series of lines.

equal to the standard horizontal line. This repeats Case 3 with the positions of the standard and variable lines reversed. The illusion is 4.4%, as in Case 3.

Case 5. Method of selection: to select from the series of horizontal lines the one equal to the standard vertical line. The method of procedure was the same as in Series IV. From the result, which is +11%, it is seen that the apparent illusion of the vertical is much stronger with this method than with the two just described. The reason for this is brought out in connection with Series VII, where the effect of turning around from one form to the other is considered.

TABLE XVI.

Obs	1		3		5		2		4		6		8		Ave					
Case	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*Il*	*%Il*	*%D*	*Ce*
1	113	2	118	2	117	2	127	2	115	3	117	3	120	5	118	3	+ 4	3.5	1.3	1
2	111	2	111	4	110	1	112	3	110	3	108	2	114	3	111	2	— 3	2.6	0.9	2
3	117	2	119	0	118	2	121	2	119	0	120	2	118	2	119	1	+ 5	4.4	0.9	3
4	112	3	109	0	109	0	110	2	108	1	108	1	110	4	109	1	— 5	4.4	0.9	4
5	120	1	132	4	133	6	125	3	127	4	129	8	130	3	128	4	+14	12.0	1.3	5
6	125	6	135	5	145	2	131	3	136	2	133	7	128	8	133	5	+19	17.0	4.4	6
7	117	3	128	4	132	3	122	3	127	4	121	8	124	8	124	5	+10	8.7	3.5	7
8	117	3	135	4	142	3	128	3	133	6	130	5	123	5	130	4	+16	14.0	5.2	8
9	116	3	130	3	130	6	122	3	125	2	123	6	127	5	125	4	+11	9.6	3.5	9
10	118	5	134	4	131	3	127	3	140	3	129	6	120	6	128	4	+14	12.0	5.2	10
11	114	4	122	4	128	4	121	2	131	5	122	4	120	7	123	4	+ 9	8.0	3.5	11
12	264	3	234	3	268	3	256	3	256	3	260	12	237	9	254	5	+26	11.4	4.4	12
13	264	8	239	5	269	1	257	2	249	6	267	8	243	12	255	6	+27	12.0	4.4	13
14	255	8	234	7	262	11	255	2	250	8	269	5	242	10	252	7	+24	10.5	4.0	14
15	128	4	145	2	147	3	132	5	148	2	140	5	138	4	138	4	+24	21.0	5.2	15
16	122	3	138	4	142	6	132	3	143	2	128	4	138	5	135	4	+21	17.5	5.2	16
17	118	2	135	3	140	9	129	0	143	2	128	4	130	3	132	3	+18	16.0	5.2	17

Notation same as in Table I.

Another of the main purposes of this series was to study the area and volume illusions in some new forms. These forms will be considered in order.

In the cone the area and volume illusions enter, tending to produce an overestimation of its size. The Müller-Lyer illusion is in all probability very strong for the altitude, but it is uncertain whether or not it has any effect upon the diameter of the base. The +16% of Case 6, where the altitude was compared with a horizontal line, represents the final balancing of the illusion of the vertical (+), of the area illusion (+), of the volume illusion (+), and of the Müller-Lyer illusion (—). That is, the three illusions outweigh the fourth by 16%. In Case 7 the diameter of the base of the cone was compared with a horizontal line. The area and volume illusions enter and there is probably a little of the Müller-Lyer effect. The line was selected 7.7% too long as a result of these illusions.

The illusions in the pyramid correspond to those in the cone. When the altitude is compared with a horizontal line (Case 8), the illusions amount to 13%, and when the base is compared with the line the illusions amount to 8.6%. The altitude of the cone is judged to be 3% greater than the altitude of the pyramid, and the base of the cone 1% less than the base of the pyramid.

In Case 10 the altitude of the triangle was compared with a horizontal line. The illusion of the vertical enters to produce an overestimation and also the area illusion with a like tendency. The Müller-Lyer illusion is doubtless involved, causing an underestimation. The final result is +11%, which represents the amount by which the illusion of the vertical and the area illusion exceed the Müller-Lyer illusion. The base of the triangle was compared with the horizontal line in Case 11. The Müller-Lyer illusion, although much reduced, is probably present. The line is selected 7% too long, which represents the area illusion minus the Müller-Lyer illusion. The base of the triangle is judged to be 0.7% less than the base of the cone and 1.6% less than the base of the pyramid. These differences are due to the volume illusion in the cone and pyramid, but they do not represent the full force of these illusions, for the Müller-Lyer illusion is involved. This illusion is stronger for the base of the cone and pyramid than for the base of the triangle, as is shown by the fact that the force of the area illusion (Case 11) is about normal, but the volume illusion in the cone and pyramid is too small.

Determinations upon the cylinder, cube, and plate were repeated so as to have measurements upon them singly in the same series in which they appeared in combinations. By comparing Cases 15, 16, and 17 of this series with Cases 27, 6, and 3 respectively of Series IV, a very close agreement will be observed.

The cone was placed upon the cylinder and the total length of the two forms was compared with a horizontal

line in Case 12. The purpose of these tests upon some of the forms in combination was to bring out variations in the illusions as a result of combining the forms. It was also desirable for purposes of comparison to make analytical studies of forms which were in a way typical of some natural objects which were being studied. The result obtained for the cone and cylinder in combination represents the effect of at least five illusions, but one of which, the Müller-Lyer illusion, causes an underestimation. The form is overestimated on account of the illusion of the vertical, the area illusion, the volume illusion, and the illusion of cylinder length. It is also possible that a sixth illusion enters; this is the illusion of length, according to which double distances are overestimated, and its ultimate effect, if it is involved, is to cause the selection of a shorter line, (—). This illusion enters into any form in which there is a motive to bisection, and it is weakest for those forms which are most easily bisected.[1] In judging the length of such a form as the cone and cylinder, the observer naturally estimates the distance by halves, and by so doing he introduces the conditions under which the illusion of length operates, and the length of the form is then overestimated. But when he bisects the linear dimension of the standard form measured, he must also bisect the compared line, and immediately the illusion of length enters into this form also, and it is likewise overestimated. However, it is easier to estimate the standard form in halves than the compared form, for the former actually consists of two pieces placed one upon the other, while the latter is simply a straight line. The illusion of length, then, would be greater in the line, because it is bisected with greater difficulty, hence a shorter line must be selected to cancel the effect of the overestimation. The influence of the illusion of length is therefore represented by the minus sign. If the observers

[1] SEASHORE and WILLIAMS, Op. cit., p. 597.

did not bisect the standard form, *i. e.*, judge it in halves, the illusion of length did not enter. In making his judgments Obs. 2 followed this method and for him all six of the illusions enter in the perception of the cone and cylinder and the other double forms. The total height of the cone and cylinder is 228 mm., but the observers selected as equal to it a line of 254 mm., or one 11% too long. (No allowance for the series of lines has been made for these double forms, for it is probably less than 1% for these longer lines). The sum of the results taken upon the forms singly is 18% of 228; that is, when the cone and cylinder are in combination, there is a reduction of the total illusion. This is probably due in part to the presence of the illusion of length in the double form.

The illusions involved in the pyramid and cube when in combination are, with the exception of the illusion of cylinder length, the same as those in the cone and cylinder. The result for Case 13, where the length was compared with a horizontal line, is 27 mm., or +12% of the standard distance. The sum of the corresponding determinations for the forms taken separately is +15% of 228, or 3% more than the result for the forms when combined.

In the combined triangle and plate the number of illusions is reduced to four: the illusion of the vertical (+); the Müller-Lyer illusion (—); the area illusion (+); and the illusion of length (—). The total result is an overestimation of 24 mm., or +10.5%, as against +13% when the measurements are made upon the single forms.

The mean variation for these double forms is greater than for the single forms because the conditions are more vague and indefinite.

A complete analysis of the illusions involved in the forms in Series V is found in Table XV. The results of this series justify the following general conclusions:

The apparent strength of the illusion of the vertical varies with the method employed.

The area illusion is demonstrated in the triangle and the combined triangle and plate; and the area and volume illusions are demonstrated in the cone, pyramid, cone and cylinder, and pyramid and cube.

The illusion of length is present in both the standard form and the compared line when there is a motive to bisection as in the double standard forms.

The total effect of the illusions in the length of the double forms (Fig. 1, Forms 9, 10, and 11) is not as great as the sum of the corresponding illusions in their components.

Series VI

Series VI, the records of which bear the date of July, 1901, was planned to show the effect of distance upon some of the illusions studied in the previous series.

The observers were six young women, members of the class in elementary psychology in the summer session, and on the whole they may be classed as observers of the second type.[1] Four trials on each case were made by each observer.

The effect of distance upon the illusion of the vertical in the square, upon the illusions in the cylinder, and upon the illusion of the vertical in the line, was studied. A, B, and C, following, refer respectively to the square, the cylinder, and the line.

A. Case 2 of Series I and II was repeated with the observers standing at different distances. The problem was to find the square plate at a distance of 1 meter (Case 1); at 3 meters (Case 2); at 6 meters (Case 3); at 9 meters (Case 4); and at 12 meters (Case 5). The results are given in Table XVII, from which it is seen that the illu-

[1] Observer 9 has astigmatic eyes. Right eye has 1.50 diopters of simple myopic astigmatism, axis 95 degrees; left eye, 1.50 diopters of simple myopic astigmatism, axis 98 degrees.

TABLE XVII.

	Case 1 (1 meter)		Case 2 (3 meters)		Case 3 (6 meters)		Case 4 (9 meters)		Case 5 (12 meters)	
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	104	0	107	2	107	0	110	1		
3	107	2	110	3	111	1	115	1	113	1
5	110	1	109	1	110	3	109	1	108	1
7	108	2	108	1	110	2	108	1	111	3
9	103	1	103	2	105	1	104	0	108	2
11	102	4	100	1	104	2	104	2		
Ave	106	2	106	2	108	2	108	1	110	2
Il	—8		—8		—6		—6		—4	
%Il	—7		—7		—5		—5		—3.5	
%D	2.6		2.6		2		2.6		1.8	

Notation same as in Table I.

TABLE XVIII.

	Case 1 (1 meter)		Case 2 (3 meters)		Case 3 (6 meters)		Case 4 (9 meters)		Case 5 (12 meters)	
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	95	1	94	0	94	0	95	1		
3	101	3	102	1	106	1	103	2	107	2
5	98	3	98	2	99	3	102	3	105	2
7	105	2	105	2	108	2	104	2	108	2
9	98	3	98	3	98	2	99	3	102	0
11	89	0	88	4	92	4	94	2	94	2
Ave	98	2	98	2	100	2	100	2.5	103	2
Il	—16		—16		—14		—14		—11	
%Il	—14		—14		—12		—12		— 9.6	
%D	3		3		4		3		3	

Notation same as in Table I.

sion of the vertical in the square of this size decreases with increase in distance, the illusion being twice as strong for the nearest as for the farthest distance.

B. Case 1 of Series I and II was repeated with the distance varied as for the plate above. For a detailed description of the test and the illusions involved in the cylinder,

the reader is referred to Series II. From the results, which are given in Table XVIII, it is seen that there is a tendency for the illusions in the cylinder to decrease with the increase in distance.

C. The purpose of this part of the series was to determine the effect of distance upon the illusion of the vertical for the line. The method of production was adopted and new apparatus devised in which the lines were represented by black watch springs. A piece of light manilla cardboard was tacked upon a frame 45 cm. by 90 cm. From the front side the ends of a section of a watch spring, 3 mm. in width, were pushed through two small slits 114 mm. apart. There was thus visible, from the front, a portion of the spring 114 mm. long, which was the standard line. The variable spring was pushed through from the back of a similar background and lay closely against the front side of it. The experimenter varied the length of the visible part of this spring as the observer directed until so much was exposed as was judged to be equal to the standard. The standard line was placed in front of the observer and a little below the level of the eyes. It was also in the horizontal position throughout the experiment. The background with the variable line rested upon an easel and was on a level with the eyes and at right angles to the line of regard. In Cases 1, 3, 5, and 7, the standard and variable lines were both horizontal, but the distances for the four cases were 3, 6, 9, and 12 meters respectively. In Cases 2, 4, 6, and 8, the distances were as above, but the variable line was vertical. The results are given in Table XIX.

It is a matter of common experience that an object at a distance appears to be smaller than one of the same size close at hand: hence in Cases 1, 3, 5, and 7, in which the illusion of the vertical is not involved, one would expect the sign of the result to be plus; that is, the farther line should be made longer in order to appear equal to the nearer one. It happens that the reverse of this occurs;

TABLE XIX.

	Case 1 (3 meters)		Case 2 (3 m'rs)		Case 3 (6 m'rs)		Case 4 (6 m'rs)		Case 5 (9 m'rs)		Case 6 (9 m'rs)		Case 7 (12 m'rs)		Case 8 (12 m'rs)	
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	118	3	101	2	105	1	91	5	105	9	84	1	101	2	87	5
3	119	3	115	9	130	11	126	5	131	5	122	10	134	5	132	10
5	106	12	107	5	105	7	103	5	98	8	101	5	98	7	105	10
7	117	9	110	1	110	11	99	3	105	6	99	1	95	8	93	4
9	104	8	102	8	92	6	106	5	96	10	101	6	99	5	107	5
11	110	5	103	6	114	9	99	10	113	5	107	2	116	8	113	6
Ave	112	7	106	5	109	8	104	6	108	7	102	4	107	6	106	7
Il	— 2		— 8		— 5		—10		— 6		—12		— 7		— 8	
%Il	— 2		— 7		— 4		— 8		— 5		—10.5		— 6		— 7	
%D	5		3		8		7		8		7		10.5		10	

Notation same as in Table I.

the signs of the results are minus signs. The explanation of this is that the observers consciously allowed for the variation in apparent size with distance.[1] They reasoned as follows: a line at a distance is really longer than it appears to be; in other words, a distant line which appears to be equal to a near line is really longer than the near line, therefore, if the distant line is to be actually equal to the near line, it must appear to be somewhat shorter, and the farther away the distant line is, the shorter it must be made. This reasoning was applied consistently, for there is a gradual increase in the amount of allowance which was made. The result for Case 1 is —2%; for Case 3, —4%; for Case 5, —5%; and for Case 7, —6%. These percentages are statements of this conscious allowance. In Cases 2, 4, 6, and 8, in addition to this conscious allowance for variation in apparent size with distance, the illusion of the vertical is involved with a tendency to cause the production of a line shorter than the standard (—). Thus there

[1] This does not apply to Observer 3, whose records indicate that she made no allowance.

are in these cases two motives for making the compared or distant line too short. The results for the four cases are respectively —7%, —8%, —10.5%, and —7%. To secure a statement of the illusion of the vertical for the different distances, the amount of allowance which was made must be eliminated from these results: 2% must be deducted from Case 2, 4% from Case 4, 5% from Case 6, and 6% from Case 8. The illusion of the vertical in the line then is 5% for Case 2, 4% for Case 4, 5.5% for Case 6, and 1% for Case 8. The results for Case 8 are probably less reliable than the others, as the compared line could not be seen with distinctness at a distance of 12 meters.

The following statements may be made from the results of Series VI:

The illusion of the vertical in the square varies with distance; it decreases with the increase in distance.

The illusions in the cylinder also decrease with the increase in distance.

There is a tendency to make a conscious allowance for variation with distance in the apparent length of the line.

No appreciable variation of the illusion of the vertical in the line, with distance, is indicated.

Series VII

In this series an analysis of the methods most frequently used was attempted. It was noticed that the illusion of the vertical for the line appeared to be larger than usual when the observer turned around ninety degrees to select from the series of lines one equal to the standard line. This is well illustrated in the first five cases in Series V. As the validity of all the conclusions drawn rested to some extent upon the reliability of the method employed, it was seen to be necessary to make a very careful study of the methods of production and selection used in the tests. The

illusion of the vertical for the line was taken as a medium for this study.

The six persons who served as observers were members of the laboratory class in experimental psychology in December, 1901. They had paid but little attention to illusions for about a year; they knew of the illusion but were not conscious of making allowance and they may therefore be classed as observers of the first type. The comment of one observer, that he had forgotten which way the illusion of the vertical ought to work in the various cases and so had considered it unsafe to try to allow for it, was a characteristic remark. When several different cases follow each other in rather rapid succession, the observer who has no practice in this experiment becomes, in a way, confused and usually abandons any attempt to react consistently against an illusion.

There were eight cases in the series and each observer made sixteen trials in each case. Half the number of trials, in the double fatigue order, were made at one sitting. In the first four cases the method of selection was employed.

Case 1. To select a horizontal line equal to the vertical standard line.

Case 2. To select a vertical line equal to the horizontal standard line.

Case 3. To select a horizontal line equal to the horizontal standard line.

Case 4. To select a vertical line equal to the vertical standard line.

The method employed in these four cases was the same as that used in Case 5 of Series V (p. 93). The observers turned upon a stool from one background to the other and selected from a series of lines one equal to the standard line. The lines were on a level with the eyes and one meter away.

In the remaining four cases the method of production was used.

Case 5. To produce a horizontal line equal to the vertical standard line.

Case 6. To produce a horizontal line equal to the standard horizontal line. The backgrounds fitted with watch springs as described for Series VI, were used. The standard and compared lines occupied the same position, relatively to each other, as in the first four cases; that is, the observer turned from one to the other.

Case 7. To produce a horizontal line equal to the vertical standard line.

Case 8. To produce a vertical line equal to the horizontal standard line.

Watch springs were again used for lines, but both the standard and variable lines were upon one background, which was 90 cm. square. Thus the effect of turning and the error due to the series of lines were eliminated. The two lines were at right angles to each other but the diagonal distance between the two adjacent ends was 114 mm.

The results for this series are given in Table XX, the study of which brings out several important facts.

The first of these to be considered is the effect of the series of lines upon the apparent length of the line. In

TABLE XX.

	Case 1		*Case 2*		*Case 3*		*Case 4*		*Case 5*		*Case 6*		*Case 7*		*Case 8*	
Obs	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
1	127	8	111	5	120	4	114	4	122	6	119	3	119	3	107	3
3	124	5	106	3	115	3	112	3	127	4	116	3	121	3	110	2
2	125	7	112	5	118	4	115	4	124	4	118	4	120	5	110	5
5	124	4	117	2	121	4	120	4	119	3	116	3	117	2	117	2
7	122	3	110	2	116	2	113	1	119	3	115	2	118	3	103	2
9	123	6	111	4	118	5	115	3	124	4	115	3	120	2	109	2
Ave	124	6	111	4	118	4	115	3	123	4	117	3	119	3	109	3
Il	+10		— 3		+ 4		+ 1		+ 9		+ 3		+ 5		— 5	
%Il	+ 9		— 2.6		+ 3.5		+ 0.9		+ 8		+ 2.6		+ 4.4		— 4.4	
%D	1		1.6		1.6		1.7		2		1.6		1		2.6	

Notation same as in Table I.

connection with Series III, it was stated that when a line stands among others in a series it looks too short by 1% and proper allowance was made for this error in every series in which it occurred. This 1% was obtained from a comparison of Cases 1 and 5, and 3 and 6 of the present series. The standard line was vertical and the compared line horizontal in both Cases 1 and 5, but in the former instance the compared line was one of a series and in the latter it stood alone. All the other conditions were as nearly the same as it was possible to make them. The difference between the results for the two cases is an indication of the effect of the series of lines. The result for Case 1 is +9% and for Case 5 it is +8%; there is a difference of 1%. Cases 3 and 6 are parallel to these two except that the illusion of the vertical is not involved in either. In both instances the standard and compared lines are horizontal, but where the compared line is one of a series (Case 3), the result is 0.9% greater than when it is alone (Case 6). A line placed in such a series as this (see p. 60) then, looks too short by 1%, and a line longer by a corresponding amount is selected by the observers.

This error due to the series of lines may be termed an illusion of position; it enters as a result of the position of the line in a series. Only one line was fixated at a time but the other lines were in the indirect field and so influenced the line attended to. It is probable that this error in the series of lines varies with different individuals, with the position (whether vertical or horizontal) of the line, and with the distance apart and arrangement of the lines upon the background, but for want of time the effect of these various conditions could not be measured. The allowance of 1% has been made in all cases into which this error enters upon the assumption that the error due to the series of lines was of approximately the same force for the several conditions noted.

The effect of turning from one background to the other was also determined. Cases 5 and 7 were alike in all respects except that in the former the observer looked from one line to the other, while in the latter both lines were in the field of vision at the same time. The illusion in Case 5 is 3.6% greater than in Case 7, and this difference must in some way be due to the turning. In Case 3 the illusion of the vertical is not involved, but, after the elimination of the error due to the series of lines, there is a residual of 2.5%, which is another statement of the effect of turning. Still another measurement of this is obtained from Case 6, where it amounts to 2.6%. These three statements of the effect of turning from the standard to the compared line are gained from independent sources and they are all in the same direction. The error is not peculiar to one method, for the same tendency is brought out both by the method of selection (Case 3) and the method of production (Case 6). It cannot be explained upon the hypothesis that the turning increases the illusion of the vertical by giving greater freedom, because the illusion of the vertical is not involved in either Case 3 or Case 6, in which this error is about the same as for those cases in which the illusion enters. The general law that the second of two equal stimuli appears greater in intensity, as in the case of sounds and weights, does not apply, for the observer looked back and forth from one line to the other, and furthermore, the error is not in the right direction to be accounted for in this manner.[1] The effect of this illusion due to turning from one line to the other, is to increase the apparent length of the standard line; this is shown by the selection or production of a line longer than the standard as equal to it. It was thought best not to make any allowance throughout the several

[1] The observer was allowed to turn back and forth and make several comparisons between the standard and the compared forms before giving his judgment. Therefore one cannot say that the standard was the first, and the compared form the second stimulus.

series for this error due to turning, as this was a constant element in all the tests and so has no effect upon the relative value of the results nor the conclusions drawn from them, other than to reduce slightly some of the percentages given.

It will perhaps be wondered why the result for Case 2, which is —3.6% after the 1% has been subtracted for the error due to the series of lines, is not the same as for Case 1, since apparently it is only the reciprocal of it. Two reasons may be given for the lack of a closer correspondence in the two cases. One is that more attention is called to the illusion of the vertical when the vertical line is varied, and there is therefore a stronger tendency to allow for the illusion. A second reason is that the apparent length of the standard line is increased by turning, and this results in the selection of a longer compared line, thus reducing the apparent force of the illusion of the vertical in Case 2.

Case 8 was introduced as a check upon Case 7, the same illusion being involved in both cases. The method of production, in which neither the error due to the series of lines nor that due to turning was involved, was used. The results for the two cases agree; and moreover they agree also with Cases 4 and 3 of Series V, in which the same method of production was used but with different apparatus and other observers.

In the method of selection as most frequently used, it is found then that the error due to the series of lines is 1% (Case 3 minus Case 6); and that the effect of turning is 3.6% (Case 5 minus Case 7). By the method of production, in which these errors were not involved, the illusion of the vertical for the line is 4.4% (Cases 7 or 8). In Case 1, the illusion of the vertical, the error due to the series of lines and that due to turning are all involved. The result for this case is 9%, or an exact equivalent of the sum of these three tendencies (4.4%, 1%, and 3.6%), the statements of which were obtained from sources entirely independent of Case 1.

The following conclusions may be drawn from this series: The apparent length of the line is decreased when the line is one of a series like the one employed.

In turning ninety degrees around from the standard to the compared line, the apparent length of the standard is increased.

The illusion of the vertical is the same for either the method of production or the method of selection after the elimination of the error due to the series of lines and that due to turning. The variation shown in Series V is therefore only apparent.

Series VIII

This series was arranged to determine the effect of practice upon the illusion of the vertical for the line.[1] In this form the illusion of the vertical seems to be the only illusion involved. The records are dated January, 1902. A series of one thousand tests was made upon each of three observers. There were ten periods of practice for each observer and one hundred trials were made at each period. These ten periods of practice occurred on successive days, except Saturdays and Sundays. Each period was about an hour long and with one or two exceptions the hour of the day was the same for each observer.

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[1] Professor Judd has made a study of the influence of practice upon the Müller-Lyer illusion. Several hundred measurements were made upon each of two observers with the result that the force of the illusion decreased rapidly with practice. The nature of this improvement with practice was regarded as "a change in the perceptual process, which change has taken place through repeated efforts to deal directly with the objects perceived." This perceptual process was regarded as "uninfluenced by expectation." Psychol. Rev., 1902, IX, p. 27. The announcement of Professor Judd's conclusion in regard to the Müller-Lyer illusion was the immediate occasion for making these experiments upon the illusion of the vertical, although the study of the effect of practice was a part of the original plan.

Observer 1, a young lady, seemed to be entirely unaware of the existence of any illusion. She was a careful observer, wholly without practice, and her judgments were naive. At no time was there a suggestion made from which she could obtain any hint as to how she was progressing. At the beginning of the series she was told simply that the object of the experiment was to determine whether or not practice had any influence upon the accuracy of her judgments.

Observer 2, a man of extremely phlegmatic temperament, was an advanced student in psychology. He had studied illusions and had had some practice as an observer in some of the previous tests. He aimed to avoid recalling what he had studied in regard to illusions, and according to his own statement he made no allowance whatever. Each judgment was independent of all the preceding ones and appeared correct to him. He was not told anything about his records until all the trials were completed.

Observer 4 had the advantage of knowledge of the subject of illusions and also a considerable amount of practice as observer. He was not at all sure that the illusion would come out for him, but he endeavored faithfully to make judgments which appeared to him correct. He was told nothing about the results of any of the other observers nor of his own until all the trials were completed and his own introspective account had been written out. This account is given herewith.

"January 16, 1902. The comparison has been made by fixating the middle of each of the lines, thus bisecting them and comparing the four halves rather than the wholes.[1] I adopted this method because I had formed the habit in

[1] On page 96 it was stated that when there is a motive for bisection the illusion of length enters. This does not have any effect upon these results as the standard and compared lines are bisected with equal effort and consequently the effect of the illusion of length is eliminated.

previous experiments and it seems to me to be the easiest and most accurate method of comparing. It did not obviate eye movements. I have felt no inclination to use units of measurement. I have imagined the horizontal line turned to the vertical position and superposed upon the standard line. In this act I have felt a strong inclination to make a correction of something over 6% which I estimate my normal illusion to be under these particular conditions. This 6% is only an estimate because I have never measured my illusion under these conditions before. The new factor here is the distance between the two lines. The method of adjusting the variable line is also new. The correction tends to enter irresistibly in the perceptive process. I am unable to tell whether I have made any correction or not. But, from my point of view, the conditions have been uniform throughout each period and throughout the whole series. If the results show any systematic variation in the progress of the experiment, I think it may be due to practice. I have asked the experimenter to mark certain records with which I felt especially well satisfied. There were marked differences in the certainty that I felt."

The method of production was employed by means of a new apparatus which proved exceedingly satisfactory. The lines to be studied were represented by watch springs as in the foregoing series. (Standard length, 114 mm.; width of spring, 2 mm). The observer adjusted the length of the compared line (the adjustable spring) by moving a lever. The lever was pivoted at a point 1 meter back of the background. From this point an arm extended to the back side of the background where it was connected with a sliding guide, to which one end of the spring was attached; the other arm of the lever was 2 meters long and came within convenient reach of the observer. This enabled the observer to make his own adjustments quickly and accurately, and eliminated the personal element of the experimenter.

In the statement of the results for each observer, the one

TABLE XXI. *Observer 1*

	1st day		*2d day*		*3d day*		*4th day*		*5th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	139.7	4.3	135.5	2.6	145.2	1.8	138.6	2.4	142.4	2.8
	132.7	3.1	137.8	3.4	143.6	2.4	142.3	4.1	142.3	2.3
	134.2	4.4	136.7	1.1	144.3	2.0	141.3	2.5	141.8	3.0
	138.6	2.9	134.1	3.5	142.3	2.4	137.8	4.6	137.2	2.6
	136.3	4.0	134.9	4.5	140.3	2.3	135.4	3.2	138.0	1.2
	134.9	3.1	134.7	2.5	136.5	2.5	137.2	3.8	141.3	2.1
	138.2	4.2	134.2	3.0	138.1	2.9	139.6	2.4	138.9	2.1
	138.1	4.3	134.6	2.2	138.8	3.2	138.9	3.1	137.1	1.9
	140.3	3.5	135.5	2.5	141.3	2.1	137.7	2.7	137.1	1.3
	140.5	1.3	135.2	1.9	141.8	2.8	135.4	2.8	136.3	2.1
Ave	137.4	3.5	135.4	2.7	141.2	2.4	138.4	2.2	139.2	2.2
% Il	20.2		18.4		23.7		21		21.9	

	6th day		*7th day*		*8th day*		*9th day*		*10th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	143.3	1.7	140.6	2.2	142.9	2.4	137.6	1.6	141.8	2.0
	142.1	2.1	138.6	2.6	143.9	2.5	138.1	2.3	141.5	2.5
	140.3	3.5	132.8	1.0	138.1	2.1	137.9	1.3	139.8	2.0
	141.9	1.9	136.3	2·5	136.0	1.2	138.7	1.9	137.4	2.0
	137.9	3.0	135.8	1.2	135.2	1.4	137.0	1.8	135.8	1.6
	141.8	1.6	138.0	2.4	134.8	2.6	137.0	2.2	134.4	2.0
	139.3	2.5	137.2	2.6	137.5	1.3	134.5	2.7	139.1	3.3
	136.7	1.7	134.3	0.9	133.8	2.4	138.1	3.1	137.2	3.0
	140.0	1.8	136.3	1.7	133.6	2.2	136.2	1.8	137.3	1.9
	138.0	1.2	132.7	1.7	136.0	1.6	137.6	2.0	135.9	2.3
Ave	140.2	2.1	136.3	1.9	137.2	2.0	137.4	2.1	138.0	2.3
% Il	22.8		19 3		20.2		20.2		21	

Final average, 1000 trials, 138 mm. or an illusion of 21%. Mean variation, 2%.

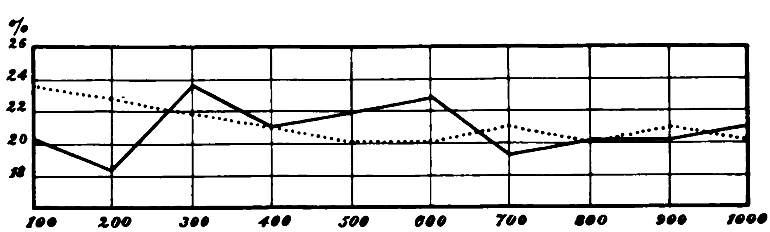

FIGURE 2

TABLE XXII. *Observer 2*

	1st day		*2d day*		*3d day*		*4th day*		*5th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	139.4	6.4	120.7	7.2	136.5	5.4	137.1	3.5	135.6	4.6
	142.0	3.6	126.1	4.7	140.7	3.1	138.4	2.0	134.6	3.0
	139.6	5.7	128.4	5.4	143.7	2.5	138.1	2.3	136.0	8.0
	146.0	3.2	126.1	2.5	144.3	3.9	142.6	1.2	145.7	3.3
	135.4	4.6	133.1	2.1	155.3	3.5	136.5	4.7	138.3	3.3
	138.0	6.2	134.0	4.0	145.0	8.4	144.6	6.6	142.8	4.4
	134.7	6.9	140.3	3.1	144.8	4.0	143.1	2.9	145.5	2.7
	138.0	5.2	138.3	4.5	141.4	3.0	139.9	5.1	142.2	3.0
	136.5	4.3	137.5	4.1	140.9	5.5	141.2	5.4	139.1	4.3
	135.6	6.0	137.4	4.4	143.9	3.9	143.7	3.9	135.9	1.3
Ave	138.5	5.2	132.2	4.2	146.7	4.3	140.5	3.8	139.6	3.8
% Il	21.9		15.8		29		23.7		22.8	

	6th day		*7th day*		*8th day*		*9th day*		*10th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	139.8	2.2	137.2	3.4	141.7	2.7	135.7	1.7	136.0	1.2
	136.4	4.4	137.1	2.9	140.9	3.5	136.0	1.1	136.3	0.8
	140.7	2.9	136.9	1.7	138.3	3.3	134.4	1.6	134.5	1.1
	140.2	3.2	141.0	4.8	132.3	1.1	134.9	1.9	137.2	2.4
	142.0	4.4	139.2	3.6	132.8	1.4	135.5	0.9	134.9	1.9
	143.6	1.4	134.5	3.5	133.1	1.5	136.2	1.7	136.2	0.8
	140.3	2.3	136.0	3.0	131.0	3.6	139.8	1.8	137.8	2.0
	136.0	3.2	135.4	4.0	133.5	0.9	137.6	1.6	134.8	1.4
	139.1	3.1	138.3	2.3	134.7	1.7	135.7	1.3	134.6	1.0
	132.9	2.7	142.2	3.1	132.9	2.5	134.6	1.6	135.4	1.0
Ave	142.7	3.0	137.8	2.9	135.1	2.2	136.0	1.5	135.8	1.4
% Il	25.4		21.9		18.4		19.3		19.3	

Final average, 1000 trials, 138 mm. or an illusion of 21%. Mean variation, 2.7%.

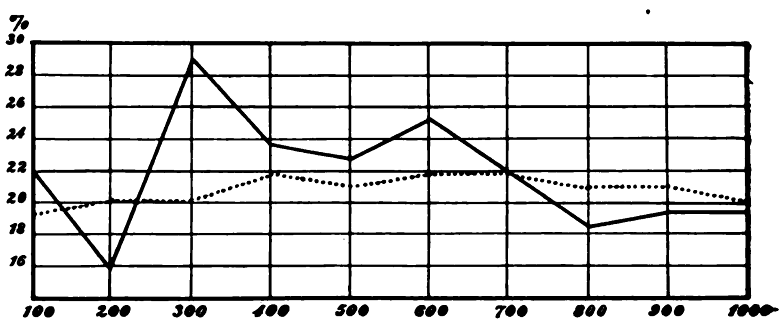

FIGURE 3

TABLE XXIII. *Observer 4*

	1st day		*2d day*		*3d day*		*4th day*		*5th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	123.9	2.2	121.2	1.0	121.0	1.6	116.4	2.8	120.2	2.2
	120.6	2.6	125.1	1.9	121.5	2.1	113.3	1.7	117.9	1.7
	121.1	1.5	119.2	1.4	123.9	1.3	114.7	0.9	117.8	1.2
	119.3	1.5	119.0	1.2	122.0	1.0	114.7	1.3	114.6	1.6
	118.1	1.3	120.1	1.3	122.5	1.6	113.8	1.6	115.4	1.4
	118.0	1.0	120.6	1.6	124.7	0.7	113.8	1.1	115.5	1.4
	117.5	1.9	120.7	1.1	124.3	0.9	114.1	0.7	121.3	2.2
	119.3	1.8	120.2	2.2	122.7	1.1	117.0	1.2	117.2	1.0
	120.5	2.1	119.2	1.2	122.7	1.7	118.7	1.7	118.6	1.6
	119.8	1.8	117.3	1.9	123.2	1.8	117.0	1.8	116.7	1.5
Ave	119.8	1.8	120.3	1.5	122.9	1.4	115.4	1.5	117.5	1.6
%Il	5.2		5.2		8.0		0.9		3.5	

	6th day		*7th day*		*8th day*		*9th day*		*10th day*	
	E	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*
	119.4	2.0	120.7	1.3	122.0	1.6	123.7	1.1	122.1	1.1
	119.4	1.0	119.3	1.4	122.4	1.0	122.7	1.9	121.8	0.8
	121.0	1.2	119.0	1.3	123.7	0.5	123.7	1.5	123.6	0.8
	121.3	1.9	119.0	2.0	126.1	1.3	123.3	1.5	124.9	1.3
	120.2	1.6	120.4	1.6	124.7	1.9	125.4	2.1	124.4	2.2
	122.5	1.1	120.7	2.3	122.3	0.9	125.5	0.8	126.4	1.6
	121.4	1.8	120.4	3.6	124.5	1.7	124.8	1.4	124.0	1.0
	121.9	2.0	121.2	1.6	123.7	0.9	124.5	1.5	124.5	1.5
	122.1	1.3	120.4	1.0	123.0	1.4	125.9	1.7	124.7	1.5
	119.7	1.1	119.0	0.8	125.0	1.9	126.0	1.4	124.0	1.4
Ave	121.0	1.5	120.0	1.7	123.7	1.3	124.6	1.5	124.0	1.3
%Il	6.1		5.2		8.7		9.6		8.7	

Final average, 1000 trials, 121 mm. or an illusion of 6.1%. Mean variation 1.4%.

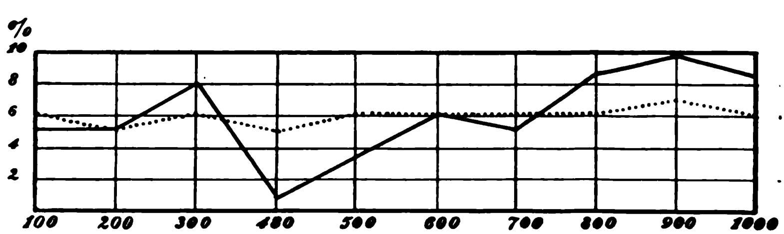

FIGURE 4

hundred trials in each practice period were divided into ten consecutive groups of ten trials each. The averages of these groups are given in Tables XXI, XXII, and XXIII, which represents respectively the judgments of Observers 1, 2, and 4. The results are represented graphically by two curves for each observer in which the numbers on the ordinates represent the percentage of illusion and the numbers on the abscissas represent the hundreds of trials. The continuous line shows the variation for the ten days and the dotted line, which is a composite of the daily curves, shows daily variation. Decrease in the force of the illusion with practice would be indicated by a fall in the curves.

The curves for Observer 1 are found in Fig. 2. The continuous line is somewhat irregular but shows no improvement as the result of practice. The dotted line indicates a slight daily improvement. The total illusion is 21%, with a mean variation of 2%.

Fig. 3 represents the curves for Observer 2. The continuous line is very irregular but it does not show any improvement worth considering. The dotted line shows no constant daily variation. The illusion, as obtained from the thousand trials, is 21%, with a mean variation of 2.7%. The strength of the illusion was very surprising to the observer himself. When shown his records and curves after the completion of the series, he was astonished at the force of the illusion, for he had been under the impression that he "was getting it exactly right."

The strength of the illusion for these two observers is unusual. It is probable that it is due to individual peculiarities, for it is known that the force of an illusion varies with different individuals, and even with the same person at different times, as the irregularities in the curves show. From previous tests it was known that the illusion for Observer 2 was strong (Series III, Observer 4), which is the reason he was chosen. The strength of the illusion for

Observer 1 was not known beforehand but it is extraordinary.

The curves for Observer 4 are given in Fig. 4. The dotted line approaches the straight line and indicates the absence of a constant daily variation. The continuous line, which represents the variation for the ten days, is rather irregular. The first thing noticed is that for the series as a whole there is no improvement as the result of practice, for the illusion at the completion of the test is fully as strong as at the beginning. On the fourth day a marked decrease in the force of the illusion is observed, for which no explanation can be offered other than that of an unconscious allowance. The observer expressly stated that he was not allowing for the illusion. At the close of the sixth period the observer watched while a few trials were made upon a little boy, and remarked that the boy should have made his estimates at least 8% shorter. By accident he saw the boy's records and was much surprised, for if the boy's estimates had been 8% smaller the illusion would not have come out at all. From this, Observer 4 concluded that he himself must have been allowing for the illusion and a rise in the curve for the subsequent tests may be due to a reaction against this. What has just been said does not apply to the first six periods and therefore fails to account for the fall in the curve on the fourth day and its rise on the fifth and sixth days. For Observer 4 the total effect of the illusion of the vertical as obtained from the average of one thousand trials is 6%, with a mean variation of 1.4%. The records with which Observer 4 was especially satisfied and which he pointed out as his best judgments are given in Table XXIV. The average illusion for these special estimates is greater than the average illusion for all the trials.

The following conclusions may be drawn from this series:

The illusion fluctuates in strength from day to day, especially for the observers who are aware of its existence.

TABLE XXIV. *Observer 4*

	1st day	*2d*	*3d*	*4th*	*5th*	*6th*	*7th*	*8th*	*9th*	*10th*
	E	*E*	*E*	*E*	*E*	*E*	*E*	*E*	*E*	*E*
	117	122	124	114	123	121	123	125	124	125
	115	120	122	116	117	120	124	123	125	120
	120	118	125	117	121	122	119	126	122	123
				122	118	123	118	128	127	122
				121	117	120	116	126	124	124
				116	117	119	123	123	127	124
				116	117	121	115	125	126	127
					118	122	125	127	124	128
					116		120	124	126	127
					115		120	125	127	126
					118		119	124	128	122
					120			126	126	126
					118			126		127
								128		125
										124
Ave	117	120	124	117	118	121	120	125	126	125
% Il	2.6	5.2	8.7	2.6	3.5	6.1	5.2	9.6	10.5	9.6

Final average, 89 trials, 121.8 mm. or an illusion of 7%. Mean variation 2%.

The practice gained in 1000 trials does not decrease the force of the illusion of the vertical for the line: this is equally true of observers who know of the illusion and of those who do not know of it.

For one observer, who has had extensive experience in the observation of this illusion for years, the illusion still has a normal force.

Series IX

The purpose of this series was to supplement Series V by measuring the illusion of the vertical in some of the standard forms for which it was not measured in Series V, and incidentally to confirm further the area and volume illusions. The tests were made in February, 1902.

One of the observers (No. 2) was Observer 4 of Series VIII, and the other five were members of the class in elementary psychology. These five observers knew something about illusions and in some cases may have made an unconscious allowance. With reference to all but the area and volume illusions the observers belong to the first type. They knew nothing about the area and volume illusions.

The method of production was used as in Series VIII. To turn from the standard form to the compared line required an excursion of the head of about 30 degrees. The standard forms were placed to the right of the observer, 1 meter away, and at right angles to the line of regard.

The different cases are described in Table XXV. In Table XXVI the signs of the various illusions involved and the percentage of illusion are given. The individual records are found in Table XXVII. Four trials were made on each case by each observer. The term 'altitude' is used with its signification in geometry as a line drawn from the apex perpendicular to the base; the altitude of a form may thus be either vertical or horizontal.

Ten of the cases from Series V were necessarily repeated and a comparative list of these is found in Table XXVIII. The methods of the two series differed in that selection was employed in Series V and production in this series. In both series the observer turned from the standard form to the compared line but in Series IX the error due to the turning is less than in Series V, because the distance between the two forms is less, and accordingly a smaller illusion is to be expected in this series. A glance at the results for the single forms in the two series shows that they agree in general, but that the illusions are a little stronger in Series V, as was expected. The records on the double forms show a distinct disagreement and at present no adequate explanation for this discrepancy can be given, except that there is great uncertainty in the perception of these double forms.

TABLE XXV.

Case	*Dimension of Standard Form Measured*	*Direction*	*No. in Fig 1*
1	Length of line	Vertical	16
2	Length of line	Horizontal	16
3	Altitude of triangle	Vertical	5
4	Altitude of triangle	Horizontal	5
5	Height of plate	Vertical	1
6	Width of plate	Horizontal	1
7	Length of triangle and plate	Vertical	9
8	Length of triangle and plate	Horizontal	9
9	Altitude of cone	Horizontal	7
10	Length of cylinder	Horizontal	3
11	Length of cone and cylinder	Horizontal	11
12	Altitude of pyramid	Horizontal	6
13	Altitude of pyramid	Vertical	6
14	Length of pyramid and cube	Horizontal	10
15	Altitude of cone	Vertical	7
16	Length of cylinder	Vertical	3
17	Length of cone and cylinder	Vertical	11
18	Length of pyramid and cube	Vertical	10
19	Height of cube	Vertical	2
20	Width of cube	Horizontal	2

In each case the compared form was a horizontal line.

Statements of the illusion of the vertical were obtained by comparing the same dimension of a form alternately in the vertical and horizontal positions with a horizontal line and taking the difference between the results of the two comparisons. For instance, when the altitude of the vertical cone is compared with a horizontal line, the illusion of the vertical is involved; but when the altitude of the horizontal cone is compared with the horizontal line, it is not involved. The difference between the two comparisons is plainly a statement of the illusion of the vertical. The other illusions change but little with the change in position of the form.

TABLE XXVI.

Case	*V*	*M-L*	*A*	*Vl*	*L*	*C-L*	*%Il*
1	+	0	0	0	0	0	+11
2	0	0	0	0	0	0	+ 5
3	+	—	+	0	0	0	+ 9
4	0	—	+	0	0	0	+ 5
5	+	0	+	0	0	0	+10
6	0	0	+	0	0	0	+ 7
7	+	—	+	0	—	0	0
8	0	—	+	0	—	0	— 2
9	0	—	+	+	0	+	+11
10	0	0	+	+	0	+	+13
11	0	—	+	+	—	+	+ 2
12	0	—	+	+	0	0	+10
13	+	—	+	+	0	0	+11
14	0	—	+	+	—	0	+ 1
15	+	—	+	+	0	+	+12
16	+	0	+	+	0	+	+19
17	+	—	+	+	—	+	+ 2
18	+	—	+	+	—	0	+ 3
19	+	0	+	+	0	0	+14
20	0	0	+	+	0	0	+11

V, illusion of the vertical.
M-L, Müller-Lyer illusion.
A, area illusion.
Vl, volume illusion.
L, illusion of length.
C-L, illusion of cylinder length.
%Il, percentage of illusion.

The following statements of the illusion of the vertical were obtained:

Line	6%	Case 1 minus Case 2
Cylinder	6%	Case 16 minus Case 10
Triangle	4%	Case 3 minus Case 4
Cube	3%	Case 19 minus Case 20
Triangle and plate	2%	Case 7 minus Case 8
Pyramid and cube	2%	Case 18 minus Case 14
Cone	1%	Case 15 minus Case 9
Pyramid	1%	Case 13 minus Case 12
Cone and cylinder	0	Case 17 minus Case 11

The conclusion from Series V, that the illusion of the vertical varies with the form, is abundantly supported by

TABLE XXVII.

Obs	2		1		4		3		6		5		Ave				
Case	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*E*	*d*	*Il*	*%Il*	*%D*
1	124	0	137	3	128	5	118	5	130	3	126	14	127	5	+13	11	3
2	116	2	127	4	117	3	115	1	127	2	118	6	120	3	+ 6	5	4
3	125	1	132	2	130	4	116	3	120	2	118	8	124	3	+10	9	5
4	120	1	126	3	130	3	110	4	118	2	118	5	120	3	+ 6	5	4
5	123	1	138	5	135	3	113	1	127	6	121	4	125	3	+11	10	6
6	120	2	128	4	140	5	109	6	122	1	113	4	122	4	+ 8	7	7
7	242	6	253	17	251	10	193	9	207	9	226	4	229	9	+ 1	0	9
8	238	6	249	7	240	6	189	10	202	4	217	6	223	7	— 5	2	10
9	122	3	138	3	132	4	114	2	123	6	129	4	126	4	+12	11	6
10	123	4	128	5	140	4	127	1	125	3	133	4	129	4	+15	13	4
11	242	1	256	13	247	11	209	14	213	8	229	10	233	10	+ 5	2	7
12	121	2	135	3	141	3	113	5	127	4	115	6	125	4	+11	10	10
13	124	3	132	4	147	2	114	2	123	8	118	4	126	4	+12	11	8
14	247	5	250	8	268	3	204	10	198	3	221	9	231	6	+ 3	1	11
15	125	4	137	3	140	2	113	4	128	7	126	9	128	5	+14	12	6
16	130	3	140	6	154	3	121	2	138	4	134	4	136	4	+22	19	7
17	248	6	242	2.	266	8	195	6	199	7	242	11	232	8	+ 4	2	10
18	154	5	246	7	271	1	192	2	192	11	251	11	234	3	+ 6	3	12
19	128	1	134	4	143	2	123	4	137	1	115	2	130	2	+16	14	7
20	120	3	128	3	149	4	118	4	132	3	110	6	126	4	+12	11	9

The notation is the same as in Table I.

the present series. Although it is difficult to group the forms in the order of their complexity, the general statement may be made that the illusion is smaller for the more complex forms, such as the cone and cylinder (Form 11), than for the simple forms, such as the line.

The presence of the area and volume illusions in this series is clearly indicated by the results. When the width of the plate is compared with the line (Case 6), the illusion of area is 7%. The differences between the results for the plate and cube give statements for the illusion of volume for the cube: 4% for both height and width (Case 19 minus Case 5, and Case 20 minus Case 6). The illusion of area for the triangle amounts to 5% plus the Müller-Lyer effect (Case 4). The volume illusion for the cone and the pyramid amounts to 6% and 5% respectively,

TABLE XXVIII.

Dimension of Standard Form Measured	*Series V*		*Series IX*	
	Case	*% Il*	*Case*	*% Il*
Length of line	5	11	1	11
Altitude of cone	6	16	15	12
Altitude of pyramid	8	13	13	11
Altitude of triangle	10	11	3	9
Length of cylinder	15	20	16	19
Height of cube	16	17	19	14
Height of plate	17	15	5	10
Length of cone and cylinder	12	11	17	2
Length of pyramid and cube	13	12	18	3
Length of triangle and plate	14	11	7	0

In this table the signs are all plus.

plus the Müller-Lyer illusion (Case 9 minus Case 4 for the cone, and Case 12 minus Case 4 for the pyramid). In the triangle and plate combined, the area illusion and the illusion of length are outweighed by the Müller-Lyer illusion by 2% (Case 8). The volume illusion for the cone and cylinder in combination is 4% (Case 11 minus Case 8) and for the pyramid and cube it is 3% (Case 14 minus Case 8).

The following points are brought out in this series:

For the single forms there is on the whole a very satisfactory agreement between this series (method of production) and Series V (method of selection).

The illusion of the vertical for the line, cone, pyramid, triangle, cylinder, cone and cylinder, pyramid and cube, and triangle and plate, is again found to vary with the form.

The area and volume illusions are further demonstrated.

The evidences of this series, together with those of Series V, indicate that the illusion of cylinder length is present to a slight degree in the cone.

Series X

The purpose of this series was to determine the variation of the illusion of the vertical with the size of the square and with the length of the line. Up to this point the chronological order has been followed in the discussion of the various series of experiments. This series, however, preceded all the others in point of time, the records being dated November, 1899. Two sets of experiments are grouped together to form this series.

The first set was made to determine the variation of the illusion of the vertical with the size of the square. The method of production was used. A large sheet of cardboard, tinted a light pink, was placed before the observer at right angles to the line of regard. In front of this was a strip of white cardboard, the width of which was the standard distance. A strip of the pink cardboard of the same width was placed over this white strip, leaving only so much of it exposed as the observer judged to be a square. The experimenter moved this pink strip as the observer, who sat 1 meter away, directed. The standards were 38, 57, 114, 228, and 456 mm.

The observers were two university professors and five members of a class in experimental psychology, all belonging to the first type. They all knew of the illusion that was being studied, but upon request they tried not to make conscious correction for it. Forty determinations were made by each observer upon the vertical position of the standard with the horizontal dimension varying (A), and forty determinations upon the horizontal position of the standard with the vertical dimension varying (B). The results for these two positions of the standard are given in percentages in Table XXIX, A and B respectively.

The illusion of the vertical is the only one that enters. With the standard vertical, it causes the width of the form to be made too large; with the standard horizontal, it

TABLE XXIX. (*A*).

Standard	*38*		*57*		*114*		*228*		*456*	
Obs	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*
2	+8	3	+4	3	+4	2	+6	1	+2	2
1	+4	4	+1	4	0	3	+1	3	+4	3
4	+1	2	−2	5	+2	4	+7	3	+4	3
6	+5	2	+4	2	+4	2	+2	2	+2	2
8	+8	3	+5	1	+7	2	+4	3	+1	2
10	+13	3	+7	4	+11	3	+7	3	+4	2
3	+6	3	0	1	+1	2	+1	1	+1	1
Ave	+6	3	+3	3	+4	3	+4	2	+3	2

TABLE XXIX. (*B*).

Standard	*38*		*57*		*114*		*228*		*456*	
Obs	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*
2	−8	2	−7	2	−4	2	−6	6	−2	2
1	−1	2	−4	3	0	2	0	2	0	4
4	−3	3	−7	2	+2	4	+4	4	−2	3
6	−3	3	−2	3	−1	1	0	1	−1	1
8	−6	0	−5	0	−4	1	−5	1	−8	1
10	+3	5	+2	2	0	2	−2	2	−3	1
3	+6	3	+2	1	+3	2	0	3	−4	2
Ave	−2	3	−3	2	−1	2	−1	3	−3	2

causes the height of the form to be made too small. That is, there should be a plus sign and a minus sign respectively for the vertical and horizontal positions of the standard.

The results reveal no definite constant tendency for the illusion of the vertical to vary with the size of the square. The observers reacted against the illusion wherever there is a minus sign in Table XXIX (A), and a plus sign in Table XXIX (B), and probably they reacted to a less extent in other instances.

The second set of experiments in this series was made at about the same time and its purpose was to determine the variation of the illusion of the vertical with the length of the line. The method of production as employed in

TABLE XXX.

Standard	*38*		*76*		*114*		*152*		*190*	
Obs	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*	*%Il*	*d*
2	−3	4	+5	1	+5	4	+11	5	+11	8
4	−1	1	+7	4	+9	10	+16	11	+23	15
6	−1	1	−1	2	−5	5	−8	7	−9	5
8	+2	2	+5	3	+4	1	+5	4	+3	6
12	+2	1	+7	3	+8	2	+15	4	+16	9
10	0	1	+4	4	+12	9	+13	9	+18	7
Ave	0	2	+5	4	+5	5	+9	7	+10	8

Case 1 of Series V was used. The standard lines were 38, 76, 114, 152, and 190 mm. in length and were in the vertical position. Twenty judgments were made upon each line by each of five observers and two judgments by each of ten other observers, all of whom belong to the first type. The records of the ten observers are grouped together and designated by the number 10. The records are summarized in Table XXX, where the percentage of illusion and the mean variation in millimeters are given. The results indicate that the illusion varies with the length of the line; for the five standard lines the illusion is respectively 0, 5%, 5%, 9%, and 10%.

It may be concluded that, for the observers in this series who tried the experiments upon the square, the illusion of the vertical does not vary constantly with the size of the square; but for those who tried the experiments upon the line, the illusion of the vertical shows a distinct tendency to increase in force with the increase in the length of the line.

Series XI

In this series the results of some experiments upon common objects are reported. The illusions which have been studied in the preceding series are not such as are found

only in a psychological laboratory; they are present in such forms as the book, box, hat, cup, barrel, coffee-pot, cupboard, churn, jug, basket, kettle, house, tree,—in fact in almost any object with which one has experience. For instance, it is well known that the height of the crown of an ordinary silk hat is greatly overestimated. This is usually attributed to the illusion of the vertical alone, but in the light of the present studies this accounts for only a part of the total illusion. The illusions in the silk hat are the same in kind as those in the vertical cylinder; the height of the hat is overestimated on account of the illusion of the vertical and the illusion of cylinder length, and the size of the hat as a whole is overestimated on account of the area and volume illusions. Experiments were planned to measure the illusions in some of these common forms, but for want of time they were not carried out. However, two excursions were made during which several large objects in the vicinity of the University campus were studied.

There were three observers in the first excursion and nine in the second; they will be referred to as the 3 Obs. and the 9 Obs., respectively. The 3 Obs., two instructors and an advanced student, were familiar with all the illusions described and therefore belong wholly to the first type.[1] The 9 Obs. were young men, members of the class in elementary psychology; with regard to the illusion of the vertical they belong to the first type, but with regard to the other illusions they belong to the second type. The two sets of observers did not all try the same tests.

In making the experiments four different methods were employed:

By the first method the required magnitude was marked off on a surveyor's steel tape. If the height of a building

[1] None of these observers knew anything in regard to whether the illusion would appear in the large objects, and the objects were not measured to determine their true sizes until after the first excursion.

was being studied, the observer was stationed at a distance from the building equal to the height. One end of the tape was fastened at the corner of the building a short distance above the ground, and an assistant held the other end so that the tape was stretched outward at right angles to the observer; the experimenter moved a pointer over the tape as the observer directed and thus marked off the distance from the corner and the number of feet was then read off from the tape. This method corresponds roughly to the method of production in the regular tests and for large objects it is sufficiently accurate.

In the second method the observer stationed himself at such a distance from the object as he considered equal to its height. This method was not very accurate and was used only to supplement the other methods.

The third method was the estimation of the ratio of the dimensions of the object. If for instance, the side of a building was being studied, the observer was asked: "If the height consists of ten units, how many such units are there in the length?"

The fourth method was to indicate upon the building a horizontal distance equal to the vertical; that is, the observer marked off a square. The experimenter stood close to the front of the building and moved a vertical pointer to the right or left as the observer directed until a distance was marked off which was judged to be equal to the height of the building.

The first object studied was the Home Education building. This is a large brick building with plain front. The stone foundation extends about four feet above the ground level and on this are four courses of red brick. From this to the eaves the brick is painted a dull gray. Counting from the top of the line of red brick to the eaves, the building is 32 feet high. This was taken as the standard distance. The length of the building is 60 feet.

Height, by the first method: The illusion of the vertical,

the area illusion and the volume illusion all tend to cause an overestimation. The 3 Obs. overestimated the height by 31%, (%D, 14); and the 9 Obs. overestimated by 34%, (%D, 8).

Height, by the second method: The same illusions are involved as with the first method but there are sources of error in the method. The 9 Obs. stood too far away by 69%, (%D, 16).

Height, by the fourth method: The illusion of the vertical is the only one entering in this instance. The 9 Obs. made the square too wide by 22%, (%D, 9).

Length, by the first method: The area and volume illusions tend to cause an overestimation. The result for the 3 Obs. is zero, (%D, 5); the result for the 9 Obs. is +18%, (%D, 8). The 3 Obs. probably made allowance for the illusions, as they knew of them, and the 9 Obs. did not.

Ratio of height to length, by the third method: According to the illusion of the vertical the height is overestimated and the units would be large, therefore a smaller number of them would be contained in the length; that is, the sign should be minus. The result for the 9 Obs. is —16%, (%D, 6).

The west side of the Physics Hall was also studied. This is a plain red brick building with a stone foundation. The height above the water-table is 43.5 feet and the length is 89 feet. The measurements upon it should be compared with the corresponding ones upon the Home Education building.

Height, by the first method: The effect of the illusion of the vertical, the area illusion and the volume illusion amounts to 18% for the 9 Obs.

Height, by the second method: In addition to large sources of error due to the method, the same illusions are involved as with the first method. The 9 Obs. stood too far away by 53%, (%D, 5).

Height, by the fourth method: The illusion of the ver-

tical caused the 9 Obs. to make the square 16% too wide, (%D, 11).

Ratio of height to length, third method: The illusion of the vertical enters with a minus sign. The result for the 9 Obs. is —19%. Five students in the department of Civil Engineering, belonging to the second type of observers, were asked to try this test. The result for them is —23%.

The excursion next proceeded to the smoke stack of the University heating plant. The chimney proper is octagonal and is built of buff pressed brick. It rests upon a square stone foundation which extends 20 feet above the ground level. The brick work above this is 103 feet, making the total height as used for the standard, 123 feet. A good unobstructed view of it was obtained from across the street, west.

Height, by the first method: The illusion of the vertical, the area and volume illusions and possibly the illusion of cylinder length enter. The 3 Obs. overestimated by 3%, (%D, 8); and the 9 Obs. by 15%, (%D, 9).

Height, by the second method: The same illusions enter as in the first method. The 3 Obs. stood back too far by 16%, (%D, 12); and the 9 Obs. by 28%, (%D, 12).

An isolated telephone pole, 31 feet high, was also measured. According to the first method the illusion of the vertical for the height of the pole for the 9 Obs. was 26%, (%D, 13).

The illusions in two large painted signs were measured by the third method. Both the signs, which were painted on the side of a two-story building, were taller than they were wide, the ratios being 19:10 and 12:10. In estimating the ratio of the width to the height, the height should be overestimated according to both the illusion of length and the illusion of the vertical. The results for the 9 Obs. were 21%, (%D, 10) and 17%, (%D, 3) for the two signs, respectively.

A very beautiful and symmetrical hard maple tree, standing somewhat isolated upon the campus, was also studied.

It had a distinct conical shape and was in full leaf. From the ground to the apex the tree measured 60 feet, and had a broadly spreading base. Just what illusions are involved in the perception of a tree has not been determined. With this particular tree the Müller-Lyer effect certainly was present (—), also the illusion of the vertical (+), the illusion of cylinder length (+), and the area and volume illusions (+). When the 9 Obs. compared the height of the tree with the tape according to the first method, they underestimated its real height by 13%, (%D, 5). These results were so striking that further experiments were made with seventeen observers, all but three of whom belonged to the second type. The results agree with those obtained above. When the height of the tree is estimated by the second method it is judged to be taller than by the first method. For the 17 Obs. the height of the tree is underestimated by 13%, (%D, 8); according to the second method it is underestimated by 3%, (%D, 9). This difference between the two methods is in accord with the differences found for the buildings and the smokestack. At various times different persons were asked to guess the height of the tree but none of them ever estimated its true height, the judgments as a rule being made too small by several feet. What motive for illusion can there be in the perception of the tree that is strong enough, in coöperation with the Müller-Lyer illusion, to counterbalance the combined effect of the illusion of the vertical, the illusion of cylinder length, the area illusion and the volume illusion? Further experiments will be made to determine this and how much may be due to the method of comparison. It seems to be a matter of common experience with those who have seen trees felled, that the tree looks much taller when standing upright than when lying upon the ground. It may be that an opinion to that effect may have led the observers to make semi-conscious or unconscious correction for it.

The stand-pipe at the C., R. I. & P. R. R. station is a

tall metal cylinder 52 feet high and 12.5 feet in diameter. It rests directly upon a concrete foundation 1 foot in height above the ground. No tall objects are near it.

Height, by the first method: The illusion of the vertical, the illusion of cylinder length, the area illusion and the volume illusion should cause an overestimation; but the result for the 3 Obs. is —9%, (%D, 4).

Diameter, by the first method: The area and volume illusions should cause an overestimation. The 3 Obs. overestimated by 12%, (%D, 0).

Ratio of diameter to height, by the third method: According to the illusion of the vertical and the illusion of cylinder length the result should have a plus sign. For the 3 Obs. the result is —9%, (%D, 15).

These results are confusing; the test must be repeated upon observers who are not aware of the illusions.

Measurements were also made upon two cylindrical oil tanks. The first tank was 20 feet in both height and diameter and had a flat roof. It corresponded to the equal cylinder in the regular experiments. The 3 Obs. were the only ones who tried the tests upon the tanks.

Height, by the first method: The illusion of the vertical, the illusion of cylinder length, the area illusion and the volume illusion all tend to cause an overestimation. The 3 Obs. overestimated by 15%, (% D, 15).

Diameter, by the first method: The illusions of area and volume enter with plus signs. The result is +10%, (%D, 10).

Ratio of diameter to height, by the third method: The illusion of the vertical and the illusion of cylinder length tend to to cause an overestimation. The illusion is 20%, (%D, 10).

The second tank was 16 feet high and 10.5 feet in diameter.

Height, by the first method: The combined effect of the illusion of the vertical, of cylinder length, of area and of volume is +13%, (%D, 19).

Diameter, by the first method: The area and volume illusions should cause an overestimation of the diameter. The diameter was overestimated by 10%, (%D, 10).

Ratio of diameter to height, by the third method: The effect of the illusion of the vertical and the illusion of cylinder length is +19%, (%D, 10).

There is great variation in these tests upon common objects. This is to be accounted for by the difference in the amount of knowledge possessed by the observer, by the individual peculiarities, the great uncertainty in the perception of the large forms, the complexity of the conditions, and the relative crudeness of the methods employed. The experiments are merely suggestive; they are too fragmentary to warrant any general conclusions, but in regard to the perception of a side of a building, two facts seem to be demonstrated:

The illusion of the vertical, the area illusion, and the volume illusion are all present.

The illusion of the vertical is greater for these large objects than for the small objects.

Critical Remarks

In the description just given of the eleven series of experiments the primary aim has been to present the full data to the reader and to place in the hands of future investigators records the significance of which has by no means been exhausted by the formal conclusions presented in this article. The writer has simply furnished the complete data and stated her conclusions as tersely as possible without elaborate discussion; the reader is referred to the tables of results for mean variations and all other such factors as enter into the interpretation of the records.

The discussions and conclusions have been based almost entirely upon the averages of the results in each series;

very little attention has been given to the records of the individual observers. A great deal might have been gained by a study of the individual observers; as for instance, a determination of the variation of the illusion with temperament and physical condition, the relative strength of the different illusions for the same person, their consistency under different conditions, variations with sex and mental ability, etc., but all this was prohibited by the closely defined limits of this report. The individual records are given in full and they may be reinterpreted at any time with these problems in view. The records also show the prevalence or constancy of the illusions for the different observers.

In many instances account has been taken of a small percentage of illusion where the corresponding mean variation has been rather large. This has been done only in those cases where there has been a clear motive for the appearance of the illusion and the variation has been normal for the particular method employed.

An apology must be given for a very rigid manipulation of the figures. In the rather intricate discussions of the results it was very desirable, for the sake of clearness, to make definite and, for the most part, unqualified statements. The fact was fully kept in mind that the results were only of relative value. The writer also regrets a certain inconsistency in the use of fractions. The apology is not that fractions were omitted in some instances, but that fractions were used at all. In the effort to interpret the results correctly this inconsistency was overlooked until the advantage to be gained from its correction would not justify the necessary expenditure of time.

Records of observers belonging to different types have in some instances been grouped together. Much confusion would have resulted from an attempt to keep the records of the different types separate. The observers as a class are representative of university undergraduates, chiefly

juniors and seniors. The individual observers have, however, been described with reference to their respective types.

With one exception, no attempt was made to determine the condition of the eyes of the observers, but no experiments were made upon any one whose eyes were obviously in a weak or strained condition. It is possible that some of the observers were troubled with errors of refraction in the eye and that the results were influenced by these defects.

The term illusion has been used in two ways: first, a motive, as when the conflict or struggle of illusions was spoken of; and second, an error, as the amount of illusion. The use of the word motive is self-evident.

General Summary

The following illusions have been considered more or less fully in the foregoing discussions: the illusion of the vertical, the area illusion, the volume illusion, the illusion of cylinder length, the Müller-Lyer illusion, and two illusions due to contiguity. Of these the Müller-Lyer illusion and the illusion of the vertical are well known. The illusion of length was reported in the article preceding this, which is really a part of the present research. The area illusion, the volume illusion, and the illusion of cylinder length have not, to the writer's knowledge, been reported before. The aim has been to make a detailed study of the illusion of the vertical, and to determine what other illusions enter into the perception of the size and form of the principal types of common objects.

A full account of the illusion of the vertical would include a discussion of the following general features: the prevalence and average strength of the illusion; variation with the form of lines and two and three dimensional ob-

jects; variation with size; variation with the distance from the observer; variations due to the angle at which the objects are seen; variations due to complications with other illusions; variation with the different standard methods of measuring illusions; variation with the age of the observers, and also with sex, intelligence, attention, environment, experience or familiarity, knowledge, practice, etc.; a complete discussion with crucial tests of the current theories to explain the illusion; and miscellaneous features, such as the duration of the stimulus, indirect vision, etc. The facts that have been determined in the present research will be summarized briefly.

The range of the illusion of the vertical is large; it varies for different observers from less than 1% to over 20%. The average illusion for the vertical line among adult observers who knew of it and were careful is about 6%. No case in which the illusion of the vertical did not appear was found which could not be accounted for by a reaction against the illusion or carelessness on the part of the observer. Very small percentages of illusion are usually to be explained in the same manner.

The illusion of the vertical varies with the form, being stronger for the less complex forms. The simplest form, the vertical line, shows the greatest illusion. The illusion is not so strong in two dimensional objects as in the line, and in three dimensional objects it is still weaker. It is probable, however, that this variation is, at least in part, only apparent; for in the complex forms there are many conflicting motives to illusion present and in their struggle for supremacy much of the manifest illusion of the vertical may be lost. It has also been pointed out in the preceding paper (page 32), that the effect of one illusion partly satisfies the motive of another. In the circle and related forms the illusion is, as a rule, practically absent on account of the knowledge of the geometrical relations of the forms. Under certain con-

ditions of attention, however, the illusion does appear even in the circle.

The illusion of the vertical varies with the size of the object. It increases in force with the increase in the length of the line; the same probably is true for two dimensional forms, although the experiments indicate no positive tendencies. The illusion is greater for the large buildings than for the small objects.

With reference to the variation of the illusion of the vertical with distance, the records show a decrease in the force of the illusion with the increase in distance. This is in accordance with the statement above that the illusion increases with increase in size; in other words, it is stronger for larger and nearer objects.

The angle at which the square is seen does not affect the illusion of the vertical in small objects.

After the errors incident to the methods have been eliminated, the illusion of the vertical is of about the same force for the different methods used.

Several subjective factors affect the illusion of the vertical. The illusion in the square is stronger for children (third type), than for adults (second type); that is, the illusion varies with age. This is due not so much to an undeveloped state of mind as to the fact that the children give unprejudiced judgments. The illusion was found not to vary with intelligence. With regard to the variation with sex there is practically no difference for the boys and girls in the force of the illusion, but among adults the women are less constant than the men. The illusion also varies with the direction of the attention. There are three ways in which the result may be influenced by the direction of the attention: the attention may be directed to the illusion itself; according to its nature, the motive of the illusion may be strengthened by attending to it or not attending to it; and the attention to one dimension of an object changes the apparent proportion of the other dimensions.

There is a greater incentive to make a correction for an illusion when the attention is strongly directed to it. The illusion of the vertical also varies with the knowledge of the illusion. It persists for some persons who have studied it for years, although they may have learned to make proper allowance for it. The illusion decreases in force when it is pointed out; it is stronger and more constant for the naive observers. The illusion was found not to decrease in force with the practice gained from making one thousand judgments. There is a very strong tendency to react against a known illusion. This illusion and other illusions also will lose much of their constancy when they become popularly known, just as the fixed customs of the savage dissolve under civilization.

A complete analysis of the area illusion would correspond somewhat closely to the one given for the illusion of the vertical. Only a few conditions under which this illusion varies have been determined. The experiments indicate that it is present in all objects having area, and its effect is to increase the apparent size of the object.[1] It is of about the same force for all the forms the areas of which are approximately equal. With the exception of two, none of the observers knew of the illusion and accordingly no conscious or unconscious correction was made for it.

In addition to the area illusion, the volume illusion appears in all forms in which the volume is either real or suggested. No constant variation with the different forms was found. As in the case of the area illusion, the observers knew nothing of the volume illusion and introduced no correction for it.

The initial purpose of this research was to study the illusion which has been termed the illusion of cylinder

[1] The existence of the area illusion may account for the fact that a line between two points looks shorter than the open distance. All the measurements upon areas in terms of lines, where the area illusion has not been eliminated, are vitiated by the existence of the area illusion.

length. The presence of this illusion of overestimation in the length of the cylinder has been fully demonstrated, but no adequate explanation for it has as yet been found. It is distinct from the illusion of the vertical and it is stronger than this illusion for adults. It is not due to one particular method, for it is brought out by the different methods used. It is not limited to the real cylinder, for it appears in the drawing of the cylinder. Neither is it due to a relative underestimation of the diameter, for it is present with its characteristic force in the tests where the ratio of the length and diameter is not considered. But these limitations do not determine the exact explanation of the illusion. The motive for eye-movement in the direction of the length of the cylinder is peculiar and seems to constitute an adequate motive for the illusion; but it has not been demonstrated by the experiments. There is an illusion in the length of the drawn cylinder (in which this motive for eye-movement is absent) equal to that in the real cylinder, but the two results are probably due to radically different causes: in the drawn cylinder it is manifestly a case of confluence due to the presence of the ellipse at the end. Association theories have been considered but it does not seem that they possess as high a degree of probability as the physiological theory. In Series I it was pointed out that this illusion varies with intelligence, being strongest for the dullest pupils; and in Series II it was shown that it did not vary with age. These two statements are in agreement, for an increase in age really means an increase in intelligence; the brightest children were probably as intelligent as the adults.

No direct determinations of the illusion of length were made, although it appeared several times in combination with other illusions. The incidental evidence which was gained supports the former report.

The Müller-Lyer illusion has figured prominently in the discussion of the results. It entered as a complicating factor

TABLE XXXI.

No. in Fig. 1	*Standard Form*	*Dimension of Standard Form Measured*	*Direction*	*A*	*Vl*	*C-L*	*M-L*	*V*	*L*
1	Plate (square)	Height	Vertical	+	0	0	0	+	0
1	Plate (square)	Width	Horizontal	+	0	0	0	0	0
2	Cube	Height	Vertical	+	+	0	0	+	0
2	Cube	Width	Horizontal	+	+	0	0	0	0
3	Cylinder	Length	Vertical	+	+	+	0	+	0
3	Cylinder	Length	Horizontal	+	+	+	0	0	0
3	Cylinder	Diameter	Vertical	+	+	0	0	+	0
3	Cylinder	Diameter	Horizontal	+	+	0	0	0	0
4	Sphere	Diameter	Vertical	+	+	0	—?	+	0
4	Sphere	Diameter	Horizontal	+	+	0	—?	0	0
5	Triangle	Altitude	Vertical	+	0	0	—	+	0
5	Triangle	Altitude	Horizontal	+	0	0	—	0	0
6	Pyramid	Altitude	Vertical	+	+	0	—	+	0
6	Pyramid	Altitude	Horizontal	+	+	0	—	0	0
7	Cone	Altitude	Vertical	+	+	+	—	+	0
7	Cone	Altitude	Horizontal	+	+	+	—	0	0
8	Disk	Diameter	Vertical	+	0	0	—?	+	0
8	Disk	Diameter	Horizontal	+	0	0	—?	0	0
9	Triangle and plate	Length	Vertical	+	0	0	—	+	+
9	Triangle and plate	Length	Horizontal	+	0	0	—	0	+
10	Pyramid and cube	Length	Vertical	+	+	0	—	+	+
10	Pyramid and cube	Length	Horizontal	+	+	0	—	0	+
11	Cone and cylinder	Length	Vertical	+	+	+	—	+	+
11	Cone and cylinder	Length	Horizontal	+	+	+	—	0	+
12	Circle	Diameter	Vertical	—	0	0	—?	+	0
12	Circle	Diameter	Horizontal	+	0	0	—?	0	0
13	Drawn square	Height	Vertical	+	0	0	0	+	0
13	Drawn square	Width	Horizontal	+	0	0	0	0	0
14	Ellipse	Long axis	Vertical	+	0	0	—	+	0
14	Ellipse	Long axis	Horizontal	+	0	0	—	0	0
15	Drawn cylinder	Length	Vertical	+	+	+?	0	+	0
15	Drawn cylinder	Length	Horizontal	+	+	+?	0	0	0
15	Drawn cylinder	Diameter	Vertical	+	+	0	0	+	0
15	Drawn cylinder	Diameter	Horizontal	+	+	0	0	0	0
16	Line	Length	Vertical	0	0	0	0	+	0
16	Line	Length	Horizontal	0	0	0	0	0	0

The notation is the same as in Table XV.

into many of the forms studied, therefore it was necessary to interpret the results with reference to this illusion.

Two illusions due to contiguity in space were brought out by the results. The first has been referred to as an error due to the use of a series of lines; the second, an error of a similar nature, due to the use of a series of plates. The effect of both was eliminated from the results.

In the way of a final summary, an analysis of the illusions involved in the sixteen forms studied is given in Table XXXI. The direction of the illusions is represented by the plus and the minus signs. The illusions are stated in terms of the horizontal line, for the method of production. The force of the illusions would vary with the numerous different conditions, but the direction of the illusions would remain as indicated in the table.

It gives the writer pleasure to acknowledge her obligation to all those who kindly served as observers, and more especially to Dr. C. E. Seashore who most generously gave his time and thought to the promotion of the research, with regard both to the planning of the tests and also to the arrangement of them in final form for presentation.

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The new Psychological Laboratory of the University of Iowa was completed and occupied in September, 1901. It is situated on the second floor of the north wing of the Hall of Liberal Arts, a new Bedford-stone structure begun in 1897. This is a fire-proof building, constructed at a cost of about $203,000, 210 feet by 120 feet, and three stories high with a basement. It is heated and ventilated by the fan system combined with direct radiation, a Sturtevant thermostat in each room maintaining the temperature at any desired degree of heat.

Previous to the drawing of the plans of the building, 4500 square feet of clear floor space were set aside for the department of philosophy and psychology, including the laboratory. Through the kindness of the Board of Regents, *carte blanche* was given to the department in conference with the architect in respect to the plans, including all details of construction and arrangement. The building committee of the Board generously granted every reasonable request to the end of securing in the outcome as perfect a laboratory as could be designed within the allotted space.

The rooms with their names and sizes are as follows:

Lecture Room	27′— 8″	×	29′—10″
Apparatus Room	15′— 7″	×	23′— 2″
Work Shop	20′— 0″	×	23′— 5″
Measuring Room	15′— 5″	×	18′— 8″
Observing Room	16′— 2″	×	12′— 7″

Private Laboratory	12′— 3″	×	20′— 0″
Research Room	16′— 2″	×	20′— 0″
Research Room	19′— 6″	×	20′— 0″
Small Lecture Room*	16′— 6″	×	23′— 9″
Library and Seminary Room*	20′— 9″	×	26′— 0″
Office	10′— 2″	×	20′— 9″
Office	9′—10″	×	23′— 2″

The arrangement of the rooms is such as secures both quiet and convenience. None of the laboratory rooms open directly upon the main hall of the building. Every room is as free as could be desired from noise and disturbance both from within and without the building, while for the two research rooms, still further seclusion is obtained by their location upon the floor above the main laboratory, from which they are reached by a private stairway from the work shop. The rooms are all thirteen feet in height, excepting the observing room which is 10 feet 2 inches in height. The floors are of maple and the other finishing is all of oak. The work shop is wainscoted in oak to the height of six feet and ceiled with oak in such a manner that, when desired, shafting, pulleys, or other fixtures may be attached for the operation of machinery. All the rooms are supplied with gas and electrical lighting. The large lecture room and the work shop are supplied with hot and cold water. The windows of all the rooms except the two offices have extra darkening shades working in grooves at the sides. These darkening shades are hung on heavy rollers boxed into the casing at the top of the window.

A switch-board is built into the wall of the work shop adjoining the battery closet. It is 2 feet by 3 feet in size,

*Owing to the recent fires, destroying two of the University buildings, these two rooms are temporarily used by another department. Meanwhile the apparatus room, which adjoins the large lecture room. is used as the library and seminary room, and one of the offices is used as apparatus room.

made of white marble set in an oak frame and contains eighty single terminals, all fused. From the switch-board electrical connections are made by concealed wires running through iron tubing in the walls and under the floors to the lecture rooms and each of the laboratory rooms, providing four circuits to each room except the observing room, which has five circuits, and the battery closet, which has six circuits. The mains from the University power-plant are also brought into the system. The switch-board is used not only for supplying the current to all the rooms but also for bringing different suites of rooms into telephonic or other electrical connection. The terminal boards in the several rooms are found at convenient places in the walls.

The construction of the observing room deserves especial mention. To make a dark room impervious to external light is a matter presenting no serious difficulty. To make a room impervious to external sound or wholly free from the jarring from surrounding rooms or adjacent streets is a problem which has not yet been solved and of course never will be. We made the attempt to approach a little nearer to this end than has hitherto been done. The result is a room as free from external disturbances as is needed in any experiments in which it is necessary to control visual, auditory and tactual stimuli. So far as this has been accomplished, the credit is largely due to the architects, Messrs. Proudfoot and Bird, who worked out many of the details of construction. The position of the observing room is central, occupying a place not otherwise desirable from lack of light. The room rests on an independent foundation, having no solid connection with the rest of the building either below, above, or on the sides. The superstructure which supports the room rests upon a sand bed and a second sand bed at a higher lever still further assists in eliminating possible jarring or sound which might be communicated from the ground. The

walls of the room itself, inside the main partitions, which separate the whole space from the surrounding apartments, are made of two four inch walls of hollow tiles separated by an air space and each covered with a thick insulating material made of sea-weed. Inside of all, the walls are plastered and then lined throughout with black broadcloth. The inside room is divided into the main observing room, 12′—2″ × 12′—7″, and a vestibule, 4′—0″ × 12′—7″. The room is entered through five doors, the outer one being an ordinary oak door and the other four specially constructed tight fitting cedar doors, covered with black cloth on the sides and edges. The doors close with strong springs and are held open by automatic catches. The floor is made of Tennessee red cedar and covered with linoleum painted black. The room is heated and ventilated by means of hot air introduced not directly but from the attic through cedar shafts provided with overlapping cloth partitions which admit the air but help to exclude sound. For very fine experiments the ventilating shaft may be closed and the room ventilated during intermissions. The room may be lighted by gas or electricity, the former being introduced through rubber tubes. The furnishing consists of black tables and chairs and a telephone connected with the measuring room. In the exclusion of sound and vibration, as well as in other respects, the room has proved to be a complete success. The loudest stentorian shouting just outside the doors is absolutely unheard within.

The work shop is supplied with a substantial work bench inclosed below for the reception of lumber; also with lathe, tool case and mimeograph, drawing materials, motors, etc., and material cases with glass doors. The measuring room is supplied with a special instrument bench. The other rooms are furnished with suitable tables, chairs, apparatus cases, a students' locker, a chart case, drawing and apparatus tables.

The library and seminary room contains the departmental library.

The equipment of the laboratory, the growth of the past twelve years, has been designed to fit the plan of instruction, according to which the student in psychology may, the first year, attend a course of lectures in which a rich collection of illustrative material is used and experiments are performed before the class by the instructor. The second year, the student may himself perform a series of model experiments, and for the third and following years he may engage in the investigation of original problems. Accordingly the equipment falls into three classes: (1) apparatus, charts and other material for use in class demonstration for the first year in general psychology; (2) a complete set of apparatus for standard exercises which constitute the laboratory course for the second year; and (3) apparatus and the varied means employed in the research work, for special tests, and for advanced demonstration experiments.